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AND

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. XIV.

(FIFTH SERIES).

NUMBER LXXXV.—JULY 1882.

	Page
Dr. E. J. Mills's Researches on Melting-point	1
Mr. C. V. Boys on Measurement of Curvature and Refractive Index. (Plate I.)	30
Professors Ayrton and Perry's Experiments on the Faure Accumulator	41
Professors Ayrton and Perry on a Simplified Dispersion-Photometer	45
MM. Warburg and v. Babo on the Connexion between Viscosity and Density in Fluids, especially Gaseous Fluids	51
Mr. F. D. Brown's Notes on Thermometry. (Plate II.)	57
Notices respecting New Books:—	
Geological and Natural-History Survey of Canada, Director's Report for 1879–80	70
Dr. Geikie's Geological Sketches at Home and Abroad	71
Proceedings of the Geological Society:—	
Prof. J. W. Judd on the Relations of the Eocene and Oligocene Strata in the Hampshire Basin	73
Science and Metaphysic, by Wyndham R. Dunstan, Vice-President of the Aristotelian Society, Demonstrator of Chemistry in the Laboratories of the Pharmaceutical Society	75
On the Depression of the Zero-point in Mercurial Thermometers, by J.-M. Crafts	76
On the Oscillations of the Plane of Polarization produced by the Discharge of a Battery: Simultaneity of the Electrical and Optical Phenomena, by D. Bichat and R. Blondlot	79

NUMBER LXXXVI.—AUGUST.

M. A. F. Sundell on Absolute Systems of Physical Units	81
Major Allan Cunningham on Moseley's Theory of Steady Flow	110
Mr. T. C. Mendenhall on the Influence of Time on the Change in the Resistance of the Carbon Disk of Edison's Tasimeter	115

Page
Mr. W. J. Lewis's Crystallographic Notes. (Plate III.) 119
Prof. R. Clausius on the Dimensions of a Unit of Magnetism in the Electrostatic System of Measures 124
M. H. Brongersma on Double Refraction, produced by Electrical Influence, in Glass and Bisulphide of Carbon. (Plate IV. fig. 5 *a*–*h*.) 127
Notices respecting New Books:—
(i) Mathematical Papers by William Kingdon Clifford; (ii) Mathematical Fragments, being Facsimiles of his unfinished Papers relating to the Theory of Graphs, by the late W. K. Clifford 135
Mr. T. Muir's Treatise on the Theory of Determinants, with graduated sets of Exercises for use in Colleges and Schools 140
Proceedings of the Geological Society:—
Mr. H. J. Johnston-Lavis on the Comparative Specific Gravities of Molten and Solidified Vesuvian Lavas .. 141
Mr. G. Attwood on the Geology of Costa Rica 141
Mr. S. V. Wood on the Newer Pliocene Period in England 142
Don M. F. de Castro on the Discovery of Triassic Fossils in the Sierra de Gador, Province of Almeria, Spain .. 146
Prof. C. Lapworth on the Girvan Succession.—Part I. Stratigraphical 147
Mr. E. Wilson on the Rhætics of Nottinghamshire 149
Dr. F. Schmidt on the Silurian and Cambrian Strata of the Baltic Provinces of Russia 150
Mr. T. F. Jamieson on the Cause of the Depression and Re-elevation of the Land during the Glacial Period .. 151
On some Explosive Alloys of Zinc and the Platinum Metals, by H. Sainte-Claire Deville and H. Debray 152
On the Reaction-current of the Electric Arc, by M. Jamin, with the assistance of M. G. Maneuvrier 154
On the Motion of a Spherical Atom in an Ideal Gas, by G. Lübeck 157

NUMBER LXXXVII.—SEPTEMBER.

MM. Elster and Geitel on the Electricity of Flame. (Plate IV. figs. 1–4) 161
Lord Rayleigh on the Equilibrium of Liquid Conducting Masses charged with Electricity 184
Lord Rayleigh on an Instrument capable of Measuring the Intensity of Aerial Vibrations 186
Dr. C. R. A. Wright on the Determination of Chemical Affinity in terms of Electromotive Force.—Part VI. 188
Mr. W. Baily on an Integrating Anemometer. (Plate V.) .. 212
Rev. O. Fisher on the Effect upon the Ocean-tides of a Liquid Substratum beneath the Earth's Crust 213

Page
Mr. J. J. Thomson on the Dimensions of a Magnetic Pole in the Electrostatic System of Units 225
Mr. F. J. Smith on a new Form of Magnetic Torsion-balance and Magnetometer 227
Notices respecting New Books:—
Dr. G. Lunge's Treatise on the Distillation of Coal-Tar and Ammoniacal Liquor 228
Mr. F. E. Hulme's Worked Examination Questions in Plane Geometrical Drawing 230
On the Duration of the Perception of Light in Direct and Indirect Vision, by Aug. Charpentier 230
On an Air-Thermometer whose Indications are independent of the Barometric Pressure, by Albert A. Michelson 233
On a Property of the Isentropic Curve for a Perfect Gas as drawn upon the Thermodynamic Surface of Pressure, Volume, and Temperature, by Francis E. Nipher 233
On the Influence of the Quantity of Gas dissolved in a Liquid upon its Surface-tension, by S. Wroblewski 236
On the Structure and Movement of Glaciers, by M. F.-A. Forel. 238

NUMBER LXXXVIII.—OCTOBER.

Mr. R. H. M. Bosanquet's Notes on Practical Electricity 241
Prof. G. Wiedemann on the Methods employed for determining the Ohm 258
Mr. L. Fletcher's Crystallographic Notes 276
Mr. E. Vansittart Neale on the Tails of Comets............ 292
Mr. Silas W. Holman on a Simple Method for Calibrating Thermometers 294
On Boltzmann's Theorem on the average Distribution of Energy in a System of Material Points 299
Mr. W. Le Conte Stevens's Notes on Physiological Optics .. 312
Notices respecting New Books:—
Mr. Latimer Clarke's Treatise on the Transit Instrument as applied to the Determination of Time 319
Geology of Wisconsin. Survey of 1873–79. Vol. III... 319
Dr. J. H. W. Stuckenberg's Life of Immanuel Kant.... 322
On the Conservation of Solar Energy, by Pliny Earle Chase, LL.D. 322
On the Appearances of the Electric Arc in the Vapour of Bisulphide of Carbon, by M. Jamin, with the assistance of M. G. Maneuvrier 324
On the Electric Resistance of Glass at Low Temperatures, by G. Foussereau 325
On the Surface-tension of some Liquids in contact with Carbonic Acid, by S. Wroblewski 327

NUMBER LXXXIX.—NOVEMBER.

	Page
Lord Rayleigh's Comparison of Methods for the Determination of Resistances in Absolute Measure	329
Messrs. C. F. Cross and E. J. Bevan on the Correlation of the Chemistry of the Carbon Compounds with the Phenomena of Life	346
Dr. O. J. Lodge on the Dimensions of a Magnetic Pole in the Electrostatic System of Units	357
Dr. Eugen Goldstein on the Electric Discharge in Rarefied Gases	366
Mr. Ernest H. Cook on Carbon Dioxide as a Constituent of the Atmosphere	387
Mr. E. B. Sargant on the Dimensions of the Magnetic Pole in Electrostatic Measure	395
Notices respecting New Books :—	
Mr. J. B. Stallo's Concepts and Theories of Modern Physics	396
On Dr. C. W. Siemens's new Theory of the Sun, by M. Faye	400
On the Connexion between the Gas-density and Stratum-interval in Geissler Tubes, by Dr. E. Goldstein	402
On the Elasticity of Rarefied Gases, by E. H. Amagat	403
On the Influence of Temperature upon the Spectra of Metalloids, by M. D. van Monckhoven	406
On a Thermoscopic Method for the Determination of the Ohm, by G. Lippmann	407

NUMBER XC.—DECEMBER.

Mr. G. H. Darwin on Variations in the Vertical due to Elasticity of the Earth's Surface	409
Rev. S. Haughton's New Views of Mr. George H. Darwin's Theory of the Evolution of the Earth-Moon System, considered as to its bearing on the question of the Duration of Geological Time	427
Prof. H. Helmholtz on Systems of Absolute Measures for Electric and Magnetic Quantities	430
Messrs. J. Trowbridge and C. B. Penrose on the Thomson Effect	440
Dr. E. Goldstein on the Reflection of Electrical Rays	449
Dr. E. Goldstein on the Influence of the Shape of the Kathode on the Distribution of the Phosphorescent Light in Geissler's Tubes	455
Notices respecting New Books :—	
J. B. Chalmers's Graphical Determination of Forces in Engineering Structures	472

Page
J. E. Steggall's Questions in Pure Mathematics proposed at the B.A. and B.Sc. Pass and Honours Examinations of the University of London, with complete Solutions. 476
Prof. J. Morris's Geological Chart 476
Proceedings of the Geological Society:—
Prof. T. G. Bonney on the Hornblendic and other Schists of the Lizard District, with some Additional Notes on the Serpentine 477
On Mr. C. W. Siemens's New Theory of the Sun, by M. G. A. Hirn 478
Reply to M. Faye's Objections to Mr. Siemens's Theory of the Sun, by C. W. Siemens.......................... 480
On a Property of the Coefficient of Absorption, by Eilhard Wiedemann 483

ERRATA.

Page 217, line 10, for $\frac{dy^2}{dx^2}$ put $\frac{d^2y}{dx^2}$.

— 221, line 4, *for* cos $(2\omega+2\delta)$ *put* sin $(2\omega+2\delta)$.

— 222, line 4, *for* $\cos^2\delta$ *put* $\cos^2 2\delta$.

— 223, line 30, for η read $\frac{g}{a\mu}\eta$ bis.

PLATES.

I. Illustrative of Mr. C. Vernon Boys's Paper on Measurement of Curvature and Refractive Index.

II. Illustrative of Mr. F. D. Brown's Notes on Thermometry.

III. Illustrative of Mr. W. J. Lewis's Crystallographic Notes.

IV. Illustrative of MM. Elster and Geitel's Paper on the Electricity of Flame, and M. H. Brongersma's on Double Refraction, produced by Electrical Influence, in Glass and Bisulphide of Carbon.

V. Illustrative of Mr. W. Baily's Paper on an Integrating Anemometer.

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[FIFTH SERIES.]

JULY 1882.

I. *Researches on Melting-point. By* Edmund J. Mills, *D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College Glasgow**.

IF we desire to ascertain the purity of a chemical substance —in other words, to establish its species—two lines of investigation are open to us. We may (1) by analytical means determine the composition, or (2) measure by physical methods some natural property of the body. Both modes of research require that what is determined—whether composition or natural property—shall be constant over a fair range of genetic condition. Both modes also involve a considerable amount of inference; but, of the two, far less is demanded by the methods of physical science, which are, as a rule, distinguished by their greater certainty because of their experimental directness. To the property of melting, these characteristics are preeminently applicable.

The accurate ascertainment of melting-point, in terms of the air-thermometer, supplies us with physical constants of considerable importance. While the readings of the mercurial thermometer are subject to grave correction—its zero, in particular, being never stationary,—it is most unlikely that the melting-point of a substance will vary in any ordinary interval of time, under any common change of pressure, or with transition to a distant latitude. Actuated chiefly by these considerations, I undertook the researches of which an account is comprised in the following sections.

* Communicated by the Author.

A. *Benzol Derivatives.*

1. *Dinitrobenzol.*—The benzol from which specimens A and C were prepared had been purified by myself, first by fractional distillation, then by fractional distillation after treatment with bromine, and, lastly, by freezing and pressure. The preparation of the dinitro-compound was effected with the aid of hydric sulphate. The crude product was washed with water, alkaline water, and water successively, and then submitted to a process of purification repeatedly adopted in these experiments.

[The process in question consists in crystallization from *distinct successive solvents*, followed each time by pressure. It is well known that small quantities of impurity are prone to cling to substances with much tenacity; but the observation has most frequently been made in connexion with some single solvent. One can readily conceive that the tenacity with which a given trace of a foreign body is held may, under such circumstances, be in effect constant. If, however, we now transfer the mixture to a new solvent, then, we may fairly presume, the trace will be in a condition of altered adhesiveness, and may be much more readily separable. It is of course not easy to decide whether this result is always attained, or whether the later solvent makes any impression at all; but it will probably be granted that the method of multiple successive solvents is in general expedient to adopt.

After each crystallization the crystals were in all cases submitted to powerful pressure between folds of carefully cleansed linen or, occasionally, of silk tissue. The surface-tension of the dissolved impurities appears to be for the most part considerable; and this, aided by mechanical compression, greatly economizes the time required in preparing a pure substance.

The pressed crystals were next reduced to a very fine powder, and dried for eleven days over oil of vitriol in the dark. Care was taken never to make determinations with substances previously melted; for it not unfrequently happens that a second melting takes place at an appreciably different temperature.]

Specimens A and C were crystallized twice from naphtha, and thrice &c. (A_3 &c., C_3 &c.) from purified alcohol. Specimen B was given me by Mr. C. E. Groves, who had prepared it from thrice frozen and pressed benzol. It was crystallized once from water, once from naphtha, once &c. (B_1 &c.) from pure alcohol. The results are contained in Table I.

Dinitrobenzol melts with some sharpness after a decided pasty stage. It strongly resists pulverization—a character more especially observable in specimen B.

TABLE I.

	A_3.	A_4.	B_1.	B_2.	B_2.	C_3.	C_4.
	89°·86	89°·71	89°·79	89°·75	89°·64	89°·73	89°·67
	·75	·71	·82	·78	·71	·62	·59
	·75	·74	·71	·70	·66	·73	·62
	·78	·68	·71	·70	·64	·76	·67
	·73	·84	·66	·70	·66	·73	·64
	·75	·76	·68	·70	·74	·70	·73
	·81	·82	·79	·67	·71	·76	·64
	·78	·79	·66	·80	·69	·73	·62
	·81	·84	·71	·78	·71	·65	·75
	·67	·84	·68	·70	·74	·78	·70
	·75	·79	·79	·70	·71	·76	·62
	·59	·82	·76	·83	·62	·73	·78
	·81	·82	·66	·72	·64	·68	·70
	·83	·71	·74	·72	......	·65	·59
Mean	89·76	89·78	89·73	89·73	89·68	89·71	89·67
Probable error ...	·012	·010	·010	·008	·007	·008	·010
Thermometer ...	2	2	2	2	3	2	2

2. *Dichlorobenzol.*—For this specimen I was indebted to Dr. Hugo Müller, who prepared it by his well-known process of chlorination, which consists in treating hydrogenated bodies with chlorine in presence of iodine. It was crystallized twice from naphtha, and four times &c. (A_4 &c.) from alcohol. Dichlorobenzol melts very suddenly.

TABLE II.

	A_4.	A_5.	A_6.	A_7.	A_8.	A_9.	A_7.
	52°·61	52°·72	52°·66	52°·74	52°·74	52°·74	52°·83
	·66	·72	·71	·72	·72	·76	·75
	·74	·70	·71	·69	·69	·72	·75
	·71	·67	·74	·69	·74	·66	·75
	·74	·75	·79	·72	·74	·72	·78
	·79	·72	·74	·69	·72	·72	·73
	·74	·75	·76	·69	·69	·64	·73
	·71	·72	·79	·72	·74	·76	·70
	·76	·72	·71	·66	·76	·76	·75
	·71	·75	·71	·69	·69	·76	·73
	·69	·77	·68	·69	·76	·76	·73
	·76	·75	·74	·72	·72	·74	·67
	·74	·72	·74	·69	·74	·72	·75
	·79	·75	·74	·66	·76	·74	·73
Mean	52·72	52·73	52·73	52·70	52·73	52·73	52·74
Probable error ...	·008	·004	·006	·004	·004	·006	·006
Thermometer ...	3	3	3	3	3	3	2

3. *Dibromobenzol.*—A was a sample prepared by heating

benzol (permanently reddened by bromine) for a short time nearly to boiling, washing and rectifying the product. The benzol thus purified was used in preparing benzol by Couper's process*; and from this product, on distillation, a residue of dibromobenzol was obtained. This was crystallized once from spirit, twice from naphtha, and thrice from spirit; at the last crystallization the substance was only partly dissolved; and the crystals obtained from the solution were alone examined. C was prepared from the monobromobenzol above referred to. The product of Couper's process always contains a little dibromobenzol, which cannot be removed in the ordinary way. This was allowed to remain. Traces of another solid impurity were precipitated by exposure to sunlight for some weeks in presence of solid potassic hydrate, followed by filtration and distillation. Finally the purified monobromobenzol was mixed with bromine in the proportion $C_6H_5Br : Br_2$, and left for some weeks in the laboratory, during which it was once heated in the water-bath. The product was washed with aqueous caustic soda, some unaltered monobromobenzol removed by partial distillation with water, and the remainder crystallized once from alcohol, once from naphtha, and once from alcohol in presence of charcoal (C_1): successive fractions (C_2 &c.) from alcohol were then taken. $C_{6\alpha}$ was obtained from a hot liquid which had deposited about half its contents, which I have named $C_{6\beta}$.

Dibromobenzol melts nearly as sharply as dichlorobenzol. It is hardly possible to trace a pasty stage.

TABLE III.

	A.	A.	C_1.	C_2.	C_3.	C_4.	C_5.	$C_{6\alpha}$.	$C_{6\beta}$.
	87°·08	87°·05	87°·10	86°·97	87°·06	87°·10	87°·04	87°·06	87°·10
	87·03	87·07	86·99	87·08	87·01	87·05	87·01	87·06	87·05
	86·87	87·03	87·12	86·97	87·03	87·10	87·01	87·03	86·94
	86·97	87·05	86·99	87·16	87·03	87·10	87·01	86·98	87·07
	87·00	87·01	87·10	87·13	87·11	87·05	87·01	87·09	87·07
	86·97	86·96	87·07	86·99	87·01	87·07	87·01	87·06	87·07
	86·95	87·01	86·97	87·08	87·01	87·04	86·96	87·06	87·05
	87·03	87·01	87·12	87·13	86·98	87·06	87·01	87·06	87·05
	87·03	87·05	86·99	87·05	87·11	87·15	86·99	87·06	87·02
	87·08	87·10	87·02	87·05	87·03	87·07	87·04	87·11	87·02
	87·00	87·01	86·99	87·10	87·01	87·13	86·99	87·03	87·02
	87·03	87·03	87·12	87·08	87·03	87·04	86·99	87·10	87·02
	86·97	87·12							
	87·03	87·09							
Mean	87·00	87·04	87·05	87·06	87·04	87·08	87·01	87·06	87·04
Probable error.	·010	·007	·011	·012	·007	·007	·004	·006	·008
Thermometer ..	2	4	2	2	2	2	2	2	2

* *Ann. Chim. Phys.* [3] lii. p. 309.

4. *Dinitrobromobenzol.*—Benzol purified by bromine distillation, and freezing, was brominated by the Couper process, and freed from dibromobenzol by heating to the boiling-point with a mixture of Nordhausen and common oil of vitriol; this treatment was followed by washing and distillation.

Sample A was prepared from the above bromobenzol by Kekulé's method*. The crude product amounted to 148 per cent., theory requiring 157 per cent.: it contained a small quantity of an oily body. This was crystallized twice from naphtha and five times &c. (A_5 &c.) from alcohol.

The crystals of dinitrobromobenzol are, as observed by Kekulé, remarkably large and well defined. They powder harshly, like rosin. The concentrated alcoholic solution emits sound as it crystallizes.

Dinitrobromobenzol melts with considerable sharpness.

Table IV.

	A_5.	A_6.	A_7.	A_8.	A_9.	A_{10}.	A_{10}.
	70°53	70°54	70°56	70°68	70°67	70°68	70°56
	·53	·51	·64	·55	·54	·71	·59
	·50	·65	·61	·55	·64	·55	·53
	·53	·49	·64	·55	·67	·71	·53
	·48	·51	·56	·57	·54	·55	·56
	·53	·57	·67	·68	·70	·60	·66
	·61	·51	·59	·57	·56	·60	·66
	·58	·65	·59	·68	·64	·63	·56
	·66	·59	·48	·57	·59	·68	·46
	·53	·57	·67	·68	·59	·60	·69
	·64	·54	·69	·63	·67	·57	·71
	·48	·54	·67	·65	·62	·68	
	·50	·67	·64	·63	·67	·57	
	·56	·62	·67	·63	·67	·57	
Mean	70·55	70·57	70·62	70·61	70·62	70·62	70·59
Probable error ...	·010	·010	·010	·009	·009	·010	·015
Thermometer ...	2	2	2	2	2	2	3

5. *Nitrodibromobenzol.*—Sample A, from (D) dibromobenzol (q. v.) which had been crystallized once from alcohol, once from naphtha, and once from alcohol. 25 grm. of substance were gently heated with 250 cub. cent. of "fuming nitric acid," and allowed to cool during a rather longer period. The product, purified by means of water and ammonia, contained a little oily impurity, and weighed 28·5 grm. (=114 per cent., theory requiring 119·1 per cent.): it was crystallized twice from naphtha, and thrice &c. (A_3 &c.) from alcohol.

* *Ann. Chem. Pharm.* cxxxvii. p. 167.

B was prepared from another portion of (D) dibromobenzol. 25 grm. were gently heated for two hours with 250 cub. cent. nitrate as before, and allowed to cool during twenty-one hours. The pressed product, which had at first contained an oily body, weighed 17 grm. (=68 per cent.): it was crystallized twice from naphtha, and thrice &c. (B_3 &c.) from alcohol.

The melting-point of nitrodibromobenzol is satisfactorily sharp.

Table V.

	A_3.	A_4.	A_5.	B_3.	B_4.	B_4.	B_5.
	83°·49	83°·43	83°·46	83°·46	83°·49	83°·50	83°·32
	·53	·48	·46	·49	·49	·52	·49
	·47	·46	·51	·46	·44	·50	·44
	·55	·54	·43	·43	·54	·45	·47
	·47	·46	·49	·46	·49	·52	·49
	·55	·43	·43	·44	·46	·48	·55
	·53	·48	·51	·39	·57	·52	·57
	·53	·54	·49	·49	·52	·50	·49
	·47	·54	·49	·46	·52	·50	·41
	·49	·48	·46	·52	·52	·60	·52
	·53	·51	·49	·44	·41	·50	·52
	·55	·51	·49	·46	·49	·52	·49
Mean	83·51	83·49	83·48	83·46	83·49	83·51	83·50
Probable error ...	·006	·007	·005	·006	·008	·006	·008
Thermometer ...	2	2	2	2	2	3	2

B. *Aniline Derivatives.*

1. *Monochloraniline.*—Sample A was prepared from chlorinated acetanilide*, free from toluol. It was purified by solution in aqueous hydric chloride, evaporating thrice to dryness after filtration, precipitating with ammonia, and distilling in a current of steam. The product was crystallized twice from naphtha, and thrice &c. (A_3 &c.) from alcohol.

B was prepared more directly from aniline by the following method:—Half a pound of aniline, purified by cohobation for several hours with one sixteenth of its weight of mercuric chloride, was mixed with a pound of glacial acetate, and chlorine led slowly over its surface. Considerable heat was evolved. When this sign of action ceased to be manifested the mixture was allowed to cool, and the now solid product heated with water and caustic soda: the oily layer thus formed was withdrawn, and cohobated for a few hours with alcoholic potash. Water was next added to this solution and its residue; and the

* For the details of the methods of obtaining aniline derivatives from acetanilide see Proc. Roy. Soc. x. p. 589, and Phil. Mag. 1875, xlix. p. 21.

united amines thereby precipitated were separated by appropriate treatment with aqueous hydric chloride*. B was crystallized twice from naphtha, and thrice &c. (B_3 &c.) from alcohol.

The chlorination of acetanilide yields but little dichloraniline; the chlorination of the acetate still less.

Monochloraniline produces great cold when dissolved in alcohol. On the application of heat to its powder, it melts with great sharpness.

TABLE VI.

	A_3.	A_4.	A_5.	A_6.	B_4.	B_7.	B_8.	B_3.	B .
	69°·66	69°·68	69°·69	69°·66	69°·75	69°·68	69°·69	69°·69	69°·66
	·74	·76	·69	·63	·64	·63	·66	·66	·56
	·74	·71	·74	·63	·64	·63	·72	·63	·56
	·58	·71	·74	·66	·70	·65	·74	·66	·48
	·74	·65	·66	·79	·64	·65	·72	·71	·51
	·71	·76	·69	·69	·62	·65	·69	·66	·56
	·66	·71	·64	·71	·64	·71	·72	·58	·53
	·66	·65	·64	·66	·70	·71	·66	·61	·53
	·71	·63	·64	·63	·64	·73	·69	·66	·53
	·69	·73	·64	·66	·64	·63	·64	·66	·48
	·69	·68	·74	·71	·64	·73	·66	·63	·48
	·79	·76	·66	·77	·67	·63	·69	·71	·58
	·61	·68	·64	·77					
	·77	·63	·72	·71					
Mean	69·69	69·69	69·68	69·69	69·66	69·67	69·69	69·65	69·54
Probable error.	·010	·008	·007	·009	·007	·008	·006	·007	·010
Thermometer. .	2	2	2	2	2	2	2	2	3

2. *Trichloraniline.*—A was obtained by chlorinating a solution of aniline in glacial acetate, as described under *Monochloraniline.* The product was melted under aqueous potash, twice distilled in a current of steam, and crystallized twice from naphtha and thrice &c. (A_3 &c.) from alcohol.

The chlorination of acetanilide suspended in water yields little or no trichloraniline.

Lesimple† describes a modification of trichloraniline which he prepared from nitrotrichlorobenzol by reduction. It has, he states, a very unpleasant and persistent smell, and melts at 96°·5. On these two points my derivative differs from his: it has a faint but not unpleasant odour, and melts at about 77°. In all the other reactions and characters mentioned by Lesimple the two bodies exhibit a complete agreement. Beilstein (who,

* *Loc. cit.* † *Ann. Chem. Pharm.* cxxxvii. pp. 126, 127.

subsequently to myself, published an account of the derivative having the lower melting-point) confirms my determination. The substance with which I have dealt seems, then, to be isomeric with Lesimple's. Trichloraniline yields a non-electric powder, which, in consequence of the woolliness of this body, is rather difficult to obtain. The powder cakes somewhat on drying. The melting-point is exceedingly sharp.

TABLE VII.

	A_3.	A_4.	A_5.	A_6.	A_7.	A_8.	A_9.	A_9.
	77°·06	77°·05	77°·08	77°·06	77°·07	77°·03	77°·02	77°·00
	·03	·05	·05	·03	·07	·03	77·02	·02
	·01	·05	·05	·03	·07	·03	77·05	·02
	·06	·08	·08	·09	·04	·06	77·07	·02
	·06	·05	·08	·06	·07	·06	77·02	·02
	·09	·08	·08	·06	·10	·03	77·02	·05
	·03	·08	·08	·01	·12	·06	76·99	·02
	·03	·02	·05	·06	·10	·03	76·99	·02
	·03	·05	·05	·06	·10	·03	77·07	·05
	·06	·08	·05	·09	·10	·06	77·05	·07
	·11	·08	·05	·03	·07	·03	77·10	·00
	·06	·08	·05	·03	·10	·06	77·02	·02
Mean	77·05	77·06	77·06	77·05	77·08	77·04	77·04	77·03
Probable error ...	·005	·004	·003	·005	·004	·003	·006	·004
Thermometer ...	2	2	2	2	2	2	2	3

3. *Monobromaniline.*—In order to prepare this substance, the best commercial aniline was heated to 100° for three hours with $\frac{1}{32}$ part of mercuric bromide, which gave rise to the formation of a trace of rosaniline. The liquid was for the most part distilled off, cohobated for twelve hours with an equal weight of glacial acetate, and then distilled to 130°. The residue, after treatment with hot water, and pressure, was powdered finely, and rapidly stirred in a large bulk of water; into this, 1·75 part of bromine was gradually introduced. The brominated acetanilides thus produced were decomposed with alcoholic potash, and the resulting bromanilines separated by treatment with aqueous hydric chloride. The principal product was dibromaniline.

Specimens A and B were obtained by partially attacking with alcoholic potash the mixed bromacetanilides; the unattacked residue yielded no more on renewing the treatment. They were crystallized several times from naphtha and spirit. The remainders of A and B were further crystallized thrice from spirit (A B). The mother-liquids of these preparations

were united, and their bromaniline crystallized four times from naphtha and nine times from alcohol, in the presence, during the last two operations, of animal charcoal (C). Q was obtained by directly treating aniline with bromine. It was purified by crystallization—once from naphtha and thrice from alcohol with the aid of animal charcoal (Q_1), also once more from alcohol (Q_2).

Monobromaniline may be dried over oil of vitriol; but I have preferred to use calcic chloride.

The melting-point of bromaniline is very sharp; A B proved exceptionally sudden in this respect.

TABLE VIII.

	A.	B.	AB.	C_1.	C_1.	Q_1.	Q_2.
	61°·78	61°·78	61°·76	61°·82	61°·65	61°·93	61°·94
	·78	·82	·78	·86	·81	·85	·78
	·74	·91	·76	·76	·68	·74	·97
	·89	·80	·83	·80	·89	·88	·97
	·84	·80	·83	·80	·68	·90	·97
	·85	·74	·76	·82	·70	·88	·94
	·91	·71	·85	·75	·84	·72	·83
	·80	·82	·76	·82	·78	·85	·81
	·84	·76	·76	·78	·81	·77	·83
	·87	·84	·76	·78	·84	·88	·83
	·87	·78	·76	·76	·76	·79	·81
	·84	·89	·74	·82	·84	·79	·81
	·78	·84	·74	·84	·92	·88	·78
	·84	......	·78	·76	·84	·85	·83
	......	......	......	......	·84		
Mean	61·83	61·81	61·78	61·80	61·79	61·84	61·86
Probable error ...	·008	·010	·006	·006	·014	·011	·013
Thermometer ...	4	4	4	4	2	2	2

4. *Dibromaniline* [see *Monobromaniline*].—The substance was dissolved in water containing one tenth vol. of common aqueous hydric chloride, the solution filtered cold, and mixed with sufficient ammonia.

Specimens A and B were obtained by the partial action of alcoholic potash on bromacetanilides. A was crystallized four times from naphtha and five times from alcohol; B three times from naphtha, once from spirit, twice from alcohol. C was similarly obtained; it was separated in an evaporation for monobromaniline, having been held in solution by the hydrochloride of that body: this sample was crystallized three times from naphtha and three times from alcohol, and animal charcoal was used. F and G were a result of the further action of alcoholic potash on bromacetanilides. F was sepa-

rated like C; it was crystallized three times from naphtha, four times from alcohol, and animal charcoal was used. G was treated on five successive occasions with a quantity, insufficient to dissolve it, of the dilute aqueous hydric chloride already named; the cold filtered solution was precipitated with ammonia. Of the successive precipitates (G_1, G_2, G_3, G_4, G_5), G_1, G_3, and G_5 were used for the determination of melting-point; G_1 and G_3 were crystallized twice from naphtha and thrice from alcohol, animal charcoal being present at the last operation; G_5 was crystallized twice from naphtha, and twice from alcohol in presence of animal charcoal.

The hydrochloric solution of dibromaniline already mentioned sometimes shows supersaturation.

Table IX.

	B.	A.	C.	F.	G_5.	G_1.	G_3.	G_3.
	78°·91	78°·88	78°·74	78°·82	78°·84	78°·68	78°·79	78°·92
	·94	·91	·85	·85	·73	·73	·79	·85
	·88	·85	·85	·66	·73	·71	·79	·92
	·83	·80	·71	·82	·79	·79	·76	·81
	·88	·91	·93	·79	·76	·76	·82	·81
	·91	·77	·87	·87	·68	·87	·90	·73
	·88	·72	·90	·85	·76	·73	·74	·73
	·80	·88	·87	·69	·76	·81	·82	·83
	·94	·83	·77	·93	·79	·89	·92	·94
	·78	·77	·71	·71	·87	·92	·90	·85
	·91	·69	·85	·71	·71	·84	·82	·79
	·78	·83	·90	·90	·81	·84	·74	·85
	·91	·77	·82	·87	·87	......	·92	·79
	·78	·91	·77	·90	·81	......	·82	·92
Mean	78·87	78·82	78·82	78·81	78·78	78·80	78·82	78·84
Probable error ...	·010	·012	·013	·015	·010	·014	·011	·012
Thermometer ...	2	2	2	2	2	2	2	4

5. *Tribromaniline* [see *Monobromaniline*].—Sample A was prepared from brominated phenylacetamide by distilling the crude product of the action of alcoholic potash thereon from strong aqueous hydric chloride, and adding water to the distillate.

Z was precipitated by water from some strongly hydrochloric washings, and crystallized four times from naphtha, four times from alcohol. B was crystallized from a hot mixture of ordinary aqueous hydric chloride diluted with two vols. of water, and washed with the same liquid diluted with six vols. of water. It was crystallized twice from naphtha and twice from alcohol, animal charcoal being present. Q was prepared by directly acting with bromine on aniline dissolved

in a large volume of dilute sulphuric acid. It is the characteristic product of the reaction; but a little bromaniline is also formed, with traces perhaps of dibromaniline. It was purified by extraction with aqueous hydric chloride diluted with nine vols. of water, by distillation, and by crystallization from spirit with the aid of animal charcoal. Q_1 was crystallized thrice from spirit, Q_2 four times. In the preparation of tribromaniline, whether from aniline directly or from phenylacetamide, the product is accompanied with a considerable amount of a black substance, non-volatile, and insoluble even in boiling spirit, which evolves much hydric bromide on distillation, and then carbonizes.

The determinations headed A, Z, and B were made with a thermometer protected by two glass cylinders; in the other cases the thermometer was bare.

The exact observation of the melting-point of tribromaniline is very difficult, inasmuch as the substance becomes transparent only at the edges of a bead which is for the most part dim and pasty.

TABLE X.

	A.	Z.	B.	Q_1.	Q_2.	Q_1.
	116°·42	116°·07	116°·05	116°·20	116°·14	116°·31
	·26	·12	·21	·31	·17	·31
	·28	·26	·24	·23	·28	·24
	·17	·32	·21	·15	·22	·24
	·31	·18	·19	·26	·30	·26
	·23	·23	·16	·31	·22	·19
	·27	·23	·13	·26	·19	·11
	·23	·34	·19	·28	·22	·31
	·28	·23	·13	·31	·36	·31
	·26	·12	·21	·23	·36	·26
	......	......	......	·28	·33	·29
	......	......	......	·28	·28	·24
	......	......	......	·31	·36	
	......	......	......	·31	·30	
	......	......	......	·28	·39	
Mean	116·27	116·21	116·17	116·27	116·27	116·26
Probable error ...	·013	·018	·011	·008	·013	·011
Thermometer ...	2	2	2	2	2	3

C. *Toluol Derivatives.*

1. *Nitrotoluol.*—Nitrotoluol was prepared from coal-tar toluol, which had been purified by agitation with oil of vitriol and potash successively, and by distillation. According to Beilstein's recommendation, hydric nitrate of spec. grav. 1·48 was dropped into toluol; a stream of air was kept passing through the flask, and a stream of water round it. Through

the washed mixture of liquid and solid nitrotoluol with toluol thus obtained, a current of steam was passed, to remove toluol first; then a mixture of the two nitrotoluols came over; and from this the solid was almost completely extracted by solidification in a freezing-mixture and filtering out suddenly by atmospheric pressure. For the success of this operation, distillation in a current of steam is essential.

Sample A was purified by melting with a little Nordhausen, washing with water, and crystallizing from spirit: the mother-liquids were evaporated for deposits, d_1, d_2, d_3. The results were:—

d_1.	d_2.	d_3.	
$51^{\circ}{\cdot}45$	$51^{\circ}{\cdot}18$	$51^{\circ}{\cdot}22$	Each result is a mean.
51·11	50·83 (thermometer 2)		
51·12 (recrystallized)			
51·13–51·52 (sublimed)			

It is clear that neither crystallization from spirit (even after Nordhausen) nor sublimation gives a satisfactory result.

Sample B was purified like A, twice crystallized from spirit, once from pure high-boiling-point (132°) naphtha, twice from spirit, once from spirit of wine.

Sample C was similarly purified: four successive extracts (x) were taken in a mixture of 1 spirit, 2 water. Therm. 2. The mean results were:—

x_1.	x_2.	x_3.	x_4.
44°·5	44°·9	43°·8	50°·7

This shows that the method of extracts is also a failure. x_1, x_2, x_3 were united as C_a; x_4 was set aside as C_b; C_a was twice crystallized from naphtha, once from spirit of wine. Four similar extracts were then made, united, crystallized from naphtha, C_b added, and the mixture was again crystallized, once from naphtha, once from spirit of wine: this product is termed C_c.

Sample D was thrice crystallized from naphtha, once from spirit of wine; twice, thrice, and four times from spirit of wine. Part of the crude substance of D had not been distilled. The fractions are marked D_4, D_5, &c., according to the times of crystallization.

M was prepared by dropping purified toluol into twice its bulk of nitrous or "fuming" nitrate of spec. grav. 1·48; 50 cub. cent. of toluol were added to 100 cub. cent. nitrate in each operation, which lasted 1·6 hour. It was crystallized twice from naphtha and thrice from alcohol.

TABLE XI.

	C.	C_α.	C_c.	D_4.	D_5.	D_6.	D_7.	B.
	51°·43	51°·39	51°·22	51°·22	51°·27	51°·34	51°·34	51°·31
	·29	·39	·37	·32	·10	·39	·39	·34
	·40	·46	·30	·35	·10	·17	·24	·29
	·31	·37	·40	·25	·35	·32	·39	·34
	·29	·34	·32	·37	·37	·14	·39	·21
	·31	·34	·35	·37	·35	·17	·34	·34
	·34	·24	·37	·35	·35	·19	·39	·34
	·47	·24	·48	·09	·27	·32	·39	·37
	·31	·32	·24	·35	·39	·29	·30	·16
	·24	·21	·25	·27	·39	·17	·32	·39
	·31	·26	·19	·22	·27	·27	·36	·29
	·21	·41	·14	·35	·39	·14	·30	·29
	·31	·26	·26	·14	......	·32	·22	·29
	·24	·37	......	......	......	......	......	·31
Mean	51·32	51·33	51·30	51·27	51·27	51·35	51·34	51·30
Probable error ...	·011	·013	·017	·016	·016	·016	·010	·010
Thermometer ...	2	3	2	2	3	3	3	2

2. *Dinitrotoluol.*—I have investigated the melting-point of this substance as prepared (1) directly from toluol, (2) from liquid nitrotoluol ("metanitrotoluol"), (3) solid nitrotoluol when the active masses are relatively small, and (4) solid nitrotoluol when the active masses are relatively large.

[Case I.] Dinitrotoluol was prepared from coal-tar toluol, which had been purified by agitation with oil of vitriol and potash successively, and distillation: a mixture of oil of vitriol with hydric nitrate was used. The product was pressed to free it from oily matter, and crystallized from spirit. The portions that fell successively in the act of crystallization were called A, B, C, D, and many experiments were made with them; but the results were not very accordant. The four samples were fractionally crystallized from spirit: the first deposits being called A_1, B_1, &c.; the second, A_2, B_2, &c.

Fractions of F (an old preparation) were also employed.

Melting-points having been taken, C_1, F_4, and F_3 were found to coincide, and mixed together. Four successive fractions of the mixture were taken; and after some further fractionation,

$(F_2)_2$, $(F_1)_1$, $[(F_3F_4C_1)_2]_2$, $[(F_3F_4C_1)_3]_2$, $(F_3F_4C_1)_1$ yielded *a*;

A_1, B_1, $(F_2)_1$, $[(F_3F_4C_1)_4]_2$, $[F_3F_4C_1)_4]_1$, $[(F_3F_4C_1)_3]_1$ yielded *b*;

A_2, B_2, C_2, $(F_1)_2$, $[(F_3F_4C_1)_2]_1$ yielded *c*;

D yielded *d*.

The following results were then obtained :—

Table XII.

	a.	*a.*	*b.*	*b.*	*d.*	*b.*	G.	G.	*a.*
	69°·23	69°·12	69°·22	69°·20	69°·13	69°·23	69°·15	69°·29	69°·33
	·23	·20	·19	·20	·16	·20	·20	·31	·25
	·09	·23	·22	·20	·16	·28	·42	·16	·40
	·33	·20	·22	·20	·18	·25	·29	·09	·22
	·14	·23	·09	·25	·21	·25	·34	·31	·38
	·06	·12	......	·12	·10	·12	·29	·31	·28
	......	......	......	·23	·16	·12	·15	·06	·33
	......	......	......	·23	·29	·25	·26	·06	·33
	......	......	......	·23	·24	·20	·34	·31	·38
	......	......	......	·27	·21	......	·29	·29	·17
	......	......	......	......	·16	......	·34	·14	·17
	......	......	......	......	......	......	......	·29	·40
	......	......	......	......	......	......	......	·31	
	......	......	......	......	......	......	......	·24	
Mean	69·18	69·18	69·19	69·21	69·18	69·21	69·28	69·23	69·30
Probable error .	·025	·013	·015	·007	·010	·012	·016	·017	·016
Thermometer ...	3	3	3	3	3	2	2	3	2

[Case II.] The mixture of nitrotoluols obtained by nitrating coal-tar toluol was cooled to −17°, and the solid modification (which separated in crystals) removed by sudden application of the filter-pump and subsequent fractional distillation. The product boiled at 219°–223°. 6·5 cub. cent. of substance dissolved completely in 20 cub. cent. nitrous nitrate, with

Table XIII.

	I.	II.
	68°·99	69°·24
	69·15	·19
	·12	·32
	·23	·24
	·20	·21
	·26	·13
	·23	·21
	·20	·16
	·15	·16
	·23	·00
	·26	·11
	·23	·19
	·15	·03
	·15	·08
	·18	·13
Mean	69·18	69·16
Probable error	·012	·014
Thermometer	2	2

slight evolution of heat; after four hours' contact, 150 cub. cent. water were added. The precipitate, which was solid on the following morning, was washed with warm aqueous sodic carbonate, and then weighed about 8·5 grm., or 112·5 per cent., the theoretical yield being 132·9 per cent. I will call this specimen J dinitrotoluol. It was crystallized four times from naphtha and three times from spirit. (See Table XIII.)

[Case III.] The whole of the remainder of D nitrotoluol (p. 12) was treated with nitrous hydric nitrate in the cold; 2·5 grm. of the nitrotoluol being dissolved in 39 grm. of the nitrate, with which it remained in contact $2\frac{1}{2}$ hours. The powder was very electric. I will call this specimen L. It was crystallized four times from naphtha and thrice from spirit.

The following numbers were obtained with Therm. 2:—

Table XIII a.

	69°·28
	·17
	·20
	·17
	·14
	·12
	·12
	·17
	·22
	·17
	·14
	·03
	·22
	·22
	·22
Mean	69·17
Probable error	·010
Thermometer	2

[Case IV.] When rather larger quantities are taken, though the same proportions between the reagents and time of action be observed, a different result is obtained. Thus Y was prepared from 4·5 grm. M mononitrotoluol; the crude product (122 per cent.) was purified by warming with weak aqueous potash containing ammonia, and was crystallized twice from naphtha, eight times from alcohol. Z was made from 20·5 grm. of a new stock of solid mononitrotoluol (which had been distilled in steam, frozen, pressed, and crystallized twice from naphtha and four times from alcohol); the crude product (120 per cent.) was crystallized twice from naphtha, and eight to nine times (Z_8, Z_9) from alcohol. Both samples had a faint yellowish tinge.

TABLE XIV.

	Y.	Z_8.	Z_9.
	69°·54	69°·60	69°·43
	·57	·60	·46
	·54	·57	·51
	·62	·63	·51
	·57	·57	·38
	·59	·55	·51
	·57	·57	·51
	·57	·57	·56
	·51	·65	·56
	·51	·63	·62
	·51	·60	·59
	·59	·57	·59
Mean	69·56	69·59	69·52
Probable error ...	·007	·006	·013
Thermometer ...	2	2	2

3. *Trinitrotoluol.*—A. Some crude coal-tar dinitrotoluol was heated with about five times its weight of "fuming" nitrate for more than thirty hours, but not so as to cause ebullition of the liquid. It had lost about 15 per cent. of its weight, but, as shown by its melting-point, had not been converted into trinitrotoluol.—B was lost in the process of crystallization.—C. The deposit from the nitric mother-liquid of the preparation fused at about 59°·7. The substance itself fused at about 80°·8 when crystallized nine times from spirit; and then, when kept a few days, its melting-point rose to about 181°·8. A small residue from the entire preparation, crystallized thrice from spirit, thrice from naphtha, and thrice from spirit of wine, fused at 182°·8 nearly. This body is soluble in naphtha, insoluble in cold and very sparingly soluble in hot spirit, insoluble in water or aqueous ammonia. Quantitative experiment seems to indicate that it is a compound of dinitrotoluol with trinitrotoluol. In appearance it closely resembles trinitrotoluol. Subsequent attempts to prepare it, both directly and in the nascent way, did not succeed.—D. The result of A was confirmed.

E. A specimen of coal-tar trinitrotoluol about 3 years old, yellow at the top; the melting-point of the lower and colourless part was taken. (This result will be noticed separately.) The remainder from this first determination (part being yellow) was crystallized twice from naphtha and once from spirit of wine. The results are given under the first E column; the

result of a further crystallization from spirit under the second E column; and so on.

TABLE XV.

	F_1.	F_1.	F_1.	F_2.	F_2.	E.	E.	E.	E.	K.
	78°·88	78°·86	78°·89	78°·78	78°·85	78°·79	78°·83	78°·76	78°·88	78°·87
	·91	·91	·79	·88	·85	·76	·81	·86	·78	·95
	79·05	·81	·87	·80	·83	·81	·86	·73	·80	·95
	78·78	·86	·94	·88	·93	·86	·89	·62	·70	·90
	·81	·73	·89	·88	·80	·83	·83	·78	·88	·90
	·81	·78	·84	·90	·87	·89	·89	·78	·82	·95
	·78	·78	·78	·93	·93	·65	·76	·78	·85	·84
	·81	·84	·84	·93	·93	·83	·81	·88	·80	·90
	·81	·73	·86	·80	·92	·89	·86	·78	·82	·84
	·81	·81	·92	·88	·92	·71	·97	·83	·80	·78
	·78	·81	·83	·93	·83	·86	·73	·78	·90	·84
	......	......	......	......	......	79·02	·91	·86	·82	
Mean	78·84	78·81	78·86	78·87	78·88	78·82	78·84	78·78	78·82	78·88
Probable error.	·016	·011	·010	·011	·009	·018	·012	·013	·010	·011
Thermometer...	2	2	3	3	3	2	2	2	2	2

F. Coal-tar toluol boiling at 109°·5–112°·0 was purified with oil of vitriol in the cold; it then boiled at 110°–113°. Equal volumes (300 cub. cent.) of nitrate (sp. gr. 1·48) and this substance were mixed, the latter being run in gradually. 51 cub. cent. of toluol were recovered by distillation; hence benzol was absent. The product was washed, added to 300 cub. cent. "fuming" nitrate (sp. gr. 1·5); washed, added to 4 vols. "fuming" nitrate (the latter), left 24 hours, and 1 vol. Nordhausen poured in—this being done in two operations, on account of the violent action in the cold. The yield was 190·7 per cent.; theory, 246·7 per cent. Half of the yield was crystallized from naphtha and spirit, and termed F_1. The other half was boiled for 6×6 hours with "fuming" nitrate, and termed F_2; the product was very dark, contained scarcely any acid bodies, and showed little loss when weighed. The fusion-points of F_1 and F_2 are the same.

K. Prepared from liquid coal-tar nitrotoluol which had been twice distilled, cooled to $-17°$, filtered, again distilled (under 224°); when cooled as mentioned, only a trace of crystals was deposited. 19 cub. cent. of this liquid, 150 cub. cent. "fuming" nitrate, 100 cub. cent. Nordhausen, yielded 31·5 grm. trinitrotoluol, = 142·6 per cent., theory requiring 165·7 per cent. The product was crystallized four times from

naphtha, five times from spirit; its fusion-point then agreed with that of F_1 and F_2.

Such trinitrotoluol is found with great ease, an hour's ebullition with a mixture of equal volumes of oil of vitriol and common nitrate sufficing to produce it. Now the presence of oil of vitriol is a great hindrance to making ordinary *tri*-nitrotoluol*.

M. M nitrotoluol was treated with Nordhausen and nitrous nitrate, and yielded 157·1 per cent. of product. The melting-point of this preparation having been found not quite regular, the substance was again treated with the nitrating-mixture, and crystallized twice from naphtha, and thrice (M_1), four times (M_2), &c. (&c.) from spirit.

The crystalline form of this modification of trinitrotoluol is distinct from that of F_1 or K_1, being much more prismatic and less platy.

A small preparation (L) of trinitrotoluol made from L dinitrotoluol (itself made from D nitrotoluol) was treated like M, and the melting-point also determined before complete nitration had been effected. This point was $78°{\cdot}91 \pm {\cdot}008$, after one crystallization from naphtha and two from spirit; and the prismatic character of the crystals was apparent; but the amount of substance was too little to go on with.

Table XVI.

	M_1.	M_2.	M_3.	M_4.	M_5.
	80°·49	80°·53	80°·50	80°·58	80°·55
	·36	·56	·47	·53	·52
	·52	·58	·55	·47	·55
	·49	·42	·55	·50	·47
	·52	·56	·50	·53	·60
	·46	·42	·47	·53	·58
	·49	·55	·58	·50	·55
	·49	·53	·55	·50	·55
	·54	·50	·50	·47	·52
	·54	·48	·53	·53	·58
	·49	·61	·61	·53	·58
	·49	·58	·61	·55	·55
	·52	·42	·53	·53	·60
	·54	·42	·50	·53	·58
Mean	80·49	80·51	80·53	80·52	80·55
Probable error ...	·008	·012	·008	·005	·006
Thermometer ...	2	2	2	2	2

* *Zeit. Chem.* xiii. p. 539.

Thus it is evident that trinitrotoluol prepared from the solid modification of nitrotoluol is distinct both in form and melting-point from the others ; it is also more difficult in making.

It deserves to be mentioned that the melting-point of trinitrotoluol appears to undergo a slight change under certain conditions. Thus, a perfectly colourless specimen three years old melted at 78°·76—the number of observations being 13, and the probable error 0·012. A few grammes of the specimen F_2, after exposure to light, with frequent agitation, for 13 days, during which it became mustard-yellow on the surfaces, melted at 78°·78 ; the number of observations being 14, and the probable error 0·013. This result was obtained after two crystallizations from naphtha and two from spirit, when the substance had become brilliantly white.

D. *Toluidine Derivatives.*

Toluidine.—The modification examined is solid at the ordinary temperature, and obtainable by the reduction of mononitrotoluol.

Sample H was purchased from Messrs. Hopkin and Williams. It was purified by conversion into oxalate, which salt was thrice extracted by ether and then decomposed by potash, distillation in a current of steam, and crystallization thrice from naphtha and four times &c. (H_4 &c.) from alcohol.

S was given me by Mr. Spiller. It was pressed and crystallized twice from naphtha and four to five times (S_{4-5}) from alcohol.

G was purchased from Dr. Schuchardt, of Görlitz. It was pressed, and crystallized twice from naphtha and three to four times from alcohol (G_{3-4}).

The above were ascertained to be all different preparations.

The melting-point of toluidine is difficult to observe. The substance remains for some time in the pasty stage, and then conducts heat very badly ; from this cause the melting-point may easily be overestimated. On the other hand, when the solid substance is plunged into a bath which is hotter than the real melting-point, it melts with great readiness and sharpness. Good numbers can only be obtained with finely-powdered material ; the capillary tubes must be introduced into the bath at 4° or 5° below the melting-point; and the mercury in the thermometer must rise very slowly towards the last.

Table XVII.

	H_4.	H_5.	H_6.	H_6.	S_4.	S_5.	S_6.	G_3.	G_4.
	42°·78	42°·79	42°·78	42°·69	42°·82	42°·81	42°·80	42°·67	42°·83
	·88	·74	·67	·85	·82	·86	·76	·80	·78
	·73	·61	·73	·75	·82	·83	·74	·72	·75
	·76	·77	·76	·73	·82	·76	·76	·83	·80
	·78	·71	·70	·73	·72	·79	·71	·75	·83
	·65	·66	·83	·69	·74	·83	·68	·77	·93
	·83	·69	·70	·75	·87	·76	·86	·85	·78
	·78	·77	·73	·81	·82	·83	·86	·75	·80
	·83	·71	·78	·69	·74	·79	·81		
	·83	·69	·83	·73	·84	·68	·76		
	·70	·63	·65	·79	·74	·71	·78		
	·73	·79	·70	·77	·72	·83	·66		
	......	·63	·76	·73	·69	·81	·67		
	......	·77	·62	·81	·82	·79	·76		
Mean	42·77	42·71	42·73	42·76	42·78	42·79	42·76	42·77	42·81
Probable error.	·012	·011	·011	·009	·010	·009	·011	·013	·012
Thermometer ...	2	2	2	4	2	2	2	3	3

E. *Phenol Derivatives.*

1. *α Mononitrophenol.*—By *α* mononitrophenol I mean the more volatile mononitrophenol of the two produced by treating phenol with water and nitrate according to Fritsche's method (*Journ. prak. Chem.* lxxv. p. 257). The crude stock was several times distilled.

A was crystallized from water and dried over oil of vitriol (as the rest were): it had been exposed to light (A_1); it was further and continuously exposed to light (A_2). B_1 was a partial precipitate from spirit by water, dried in the shade; the mother-liquid gave with more water a precipitate which turned brown when dried like B_1, and then melted about 0°·6 below B_1. C_1 was made like B_1, except that scarcely any thing was left in the mother-liquid. E was a total precipitate from a sodic salt which contained, as a mean of two experiments, 14·28 per cent. of sodium (theoretically 14·31): it was dried in the shade. D was twice crystallized, probably from weak spirit, and dried in shade twenty days, *i. e.* very much longer than either of the others.

The powder of *α* mononitrophenol has a tendency to cake together on keeping.

Table XVIII.

	D.	B_1.	C_1.	E.	A_1.	A_2.
	44°·31	44°·29	44°·29	44°·22	44°·28	44°·31
	·29	·29	·24	·25	·25	·31
	·24	·24	·18	·25	·31	·23
	·33	·18	·10	·22	·28	·17
	·24	·32	·29	·27	·28	·25
	·29	·26	·29	·27	·23	·39
	·37	·29	·29	·30	·28	·31
	·33	·32	·21	·22	·31	
	·24	·21	·29	·27	·31	
	·33	·32	·29	·25	·28	
	......	·21	·29	......	·28	
Mean	44·30	44·27	44·25	44·25	44·28	44·28
Probable error ...	·009	·010	·012	·005	·005	·017
Thermometer ...	2	3	3	2	3	3

2. β *Mononitrophenol.*—β mononitrophenol is the less volatile mononitrophenol of the two produced by Fritsche's process (*Journ. prak. Chem.* lxxv. p. 257). The crude stock was converted into sodic salt, washed with aqueous sodic hydrate, dissolved in water, and precipitated by hydric chloride: a sodic salt prepared from this contained 14·17 instead of 14·31 per cent. of sodium, the preparation being effected with but very little loss.

A_1 was crystallized thrice from naphtha and once from water; A_2 was the same, twice crystallized from water; B_1, B_2, B_3 were all crystallized together thrice from naphtha, in one quantity; and this was crystallized thrice from water. All the specimens were dried over oil of vitriol in the dark for about two months.

β mononitrophenol becomes pale dirty yellow on exposure to light for a day, especially if a part of it has been melted. As the coloured product probably melts below the temperature at which the uncoloured substance does; and as a broad gas-flame must tend somewhat to produce the same effect as daylight, the long stage of incipient fusion and the uncertainty of the limit are accounted for.

TABLE XIX.

	A_1.	A_2.	B_1.	B_2.	B_3.
	111°·29	111°·50	111°·37	111°·53	111°·40
	·29	·34	·60	·41	·35
	·35	·53	·51	·32	·40
	·40	·50	·41	·34	·40
	·38	·53	·41	·39	·47
	·45	·50	·48	·46	·51
	·40	·27	·37	·41	·51
	·31	·42	·51	·41	·51
	·26	·42	·58	·44	·49
	·24	·45	·46	·37	·38
	......	......	·51	......	·31
	......	......	·32		
Mean	111·34	111·45	111·46	111·41	111·43
Probable error ...	·014	·017	·016	·012	·014
Thermometer ...	3	2	3	3	3

3. *α Dinitrophenol.*—*α* dinitrophenol appears to have been first observed by Armstrong, who, however, did not obtain it in a state of purity; the exacter definition of the substance is due to Hübner and Schneider (*Zeit. Chem.* xiv. p. 524). In preparing it, I followed the method recommended by the last-named chemists, depending more especially on the insolubility of the baric derivative in boiling spirit of 90 per cent. When the baric derivative was dissolved in water and treated with aqueous hydric chloride, the hydric salt was precipitated: this was afterwards dissolved and crystallized.

α dinitrophenol has an extremely pale yellow colour when crystallized from naphtha or spirit; the powder has a deeper shade; and the solution imparts a dark-orange tint to a tissue on which it has been dried.

A, B, C, D, E were distinct preparations. A and B had been crystallized thrice from naphtha and thrice from spirit; C had been crystallized from water, aqueous potassic chloride, and twice from spirit; D had been made from a baric salt twice extracted with spirit, after which it was crystallized once from naphtha and four times from spirit*; E was formed by uniting the remainders of A, B, and C, extracting the baric salt thrice with alcohol, and crystallizing the resulting *α* dinitrophenol once from naphtha and once from spirit.

* The hot alcoholic solution was poured off from a red, quite insoluble foreign substance.

α dinitrophenol melts sharply, the pasty stage being short. According to Hübner and Schneider its melting-point is 63–64°.

TABLE XX.

	A.	A.	B.	B.	C.	D.	E.
	61·89°	61·79°	61·85°	61·82°	61·78°	61·82°	61·79°
	·80	·79	·77	·77	·75	·85	·74
	·82	·84	·77	·78	·70	·87	·77
	·80	·82	·80	·75	·73	·74	·75
	·74	·74	·82	·67	·67	·77	·72
	·89	·87	·80	·78	·81	·71	·83
	·87	·87	·77	·80	·70	·74	·85
	·74	·84	·74	·77	·75	·79	·79
	·74	·87	·82	·82	·75	·85	·77
	·73	·92	·74	·78	·75	·74	·77
	·70	·74	·85	·71	·78	·74	·75
	·73	·79	·80	·65	·70	·85	·75
	......	......	·74	·78	·78	·82	·70
	......	......	·77	·80	·81	·82	·77
Mean	61·79	61·82	61·79	61·76	61·75	61·79	61·77
Probable error ...	·013	·010	·006	·009	·007	·009	·007
Thermometer ...	4	2	2	4	2	2	4

4. β *Dinitrophenol.*—Z. The crude compound was prepared by Grüner's method, from crystalline phenol. A considerable amount of its baric salt was crystallized repeatedly from a large volume of water, and the cooled and filtered mother-liquid precipitated with hydric chloride.

Z_ϵ was the tenth precipitate; it was crystallized thrice from water.

Z_ζ was the eleventh precipitate; it was crystallized once from water.

Z_η was the twelfth precipitate; it was crystallized once from water.

Preliminary determinations of melting-point were made with the nine preceding fractions; but the numbers were not sufficiently satisfactory to warrant proceeding with purification.

Y was made by Dr. Armstrong from trinitrophenol, by way of amido-dinitrophenol; it was crystallized thrice from water.

T. For this also I am indebted to Dr. Armstrong: he had prepared it by acting with ordinary hydric nitrate on phenol. It was crystallized once from naphtha, once from alcohol, and four times from water.

In determining the melting-points of Z_η, Z_ζ, and Y, the thermometers were protected by two glass cylinders; in the other three cases the cylinders were not used.

Crystallization, powdering, desiccation, and filling of capillary tubes had to be effected either in total darkness or in a deep shade. β dinitrophenol is very nearly white; but by two hours' exposure to a somewhat gloomy atmosphere it becomes deep turmeric yellow.

β dinitrophenol melts with moderate sharpness.

Table XXI.

	Z_η.	Z_ζ.	Y.	T.	Z_ϵ.	Z_ϵ.
	111°·60	111°·63	111°·46	111°·57	111°·66	111°·65
	·60	·63	·57	·57	·64	·55
	·46	·49	·63	·49	·61	·65
	·52	·58	·68	·60	·61	·58
	·55	·63	·82	·57	·64	·53
	·63	·60	·60	·60	·80	·63
	·46	·58	·49	·57	·61	·60
	·43	·68	·57	·52	·66	·58
	·52	·52	·60	·54	·53	·53
	·63	·55	·57	·54	·66	·65
Mean	111·54	111·59	111·60	111·56	111·64	111·59
Probable error ...	·015	·012	·020	·007	·014	·010
Thermometer ...	2	2	2	2	2	3

5. *Trinitrophenol.*—Sample A was a commercial specimen. It was crystallized twice from water, once from alcohol, and again from water.

Z was prepared from dinitrophenol made by Grüner's process (*v. suprà*); the material employed was very pure, having been precipitated from a 12th (Z_η) cold aqueous extract of the crude basic salt. This was evaporated to dryness on the water-bath with a very large excess of hydric nitrate, and crystallized once from naphtha, once from ethylic alcohol, and twice from water.

O was made from phenol, containing minute amounts of the two modifications of dinitrophenol, by evaporation, as with Z. It was crystallized thrice from water, once from alcohol, and a first (O_1), second (O_2), and third (O_3) time from water.

M was similarly prepared from sodic α nitrophenate (E under α nitrophenol), and was crystallized thrice from water.

F was derived from sodic β nitrophenate (the analysis is

given under β mononitrophenol); it was crystallized once from water, once from alcohol, and again from water.

N was prepared from some α (Hübner's) dinitrophenol, for which I am indebted to Prof. Armstrong. This was purified by Hübner's two processes, and can have contained at most mere traces of its isomer. The trinitro-compound was crystallized thrice from water.

Trinitrophenol is a nearly white substance when in crystals; if very finely divided by any means, it appears a pale yellow. The powdered crystals, if exposed for two hours to indirect light on a dull morning, acquire a deep mustard-yellow colour. The crystals of the substance termed N were almost perfectly white.

The melting-point of trinitrophenol is, on the whole, not difficult to observe.

Table XXII.

	A.	Z.	O_1.	O_2.	O_3.	M.	F.	N.	N.
	121°·11	120°·95	121°·04	121°·14	121°·03	120°·99	121°·20	121°·20	121°·09
	·06	120·92	·07	121·11	120·98	121·13	121·17	120·98	120·89
	08	121·03	·01	120·97	120·98	·16	120·98	121·09	121·09
	·11	120·95	·15	121·14	121·08	·18	121·14	·12	120·94
	·03	121·06	·18	120·97	·00	·10	·09	·12	121·11
	·06	121·00	·12	121·06	·08	·16	·01	·15	121·11
	·03	120·98	·23	·14	·06	·19	·12	·12	121·14
	·06	121·20	·09	·09	·14	·07	·04	·12	120·94
	·24	121·00	·12	·09	·11	·19	·14	·15	120·99
	120·97	121·00	·09	·11	·06	·16	120·98	·17	121·07
Mean	121·07	121·01	121·11	121·08	121·05	121·13	121·09	121·12	121·04
Probable error.	·014	·016	·013	·013	·011	·013	·016	·012	·018
Thermometer...	2	2	2	2	2	2	2	2	3

F. *Naphthalin Derivatives.*

Naphthalin.—Commercial pure naphthalin, which had been twice sublimed, was digested for a few hours with strong oil of vitriol in the water-bath. After washing with water, it was distilled from caustic soda in a current of steam. The first portion of the distillate is termed A, the second B. These portions were crystallized twice from naphtha and thrice (A_3 &c.) from alcohol. Before naphthalin melts it exhibits a decided pasty stage; yet the melting-point is sufficiently sharp. The powdered substance is highly electric.

Table XXIII.

	A_3.	A_4.	A_5	A_6.	B_4.	B_5.	B_6.
	80°·06	80°·03	80°·01	80°·03	80°·05	80°·05	80°·06
	79·98	·06	·06	·08	·05	·08	·14
	80·01	·08	·09	·00	·05	·05	·09
	79·95	·00	·06	·08	·05	·02	·09
	80·03	·00	·09	·08	·05	·08	·09
	80·06	·00	·06	·08	·07	·05	·09
	80·09	·08	·12	·08	·07	·08	·03
	80·06	·06	·12	·11	·05	·08	·09
	80·03	·11	·06	·11	·07	·05	·09
	79·98	·03	·09	·06	·07	·05	·09
	80·06	·11	·06	·06	·07	·10	·11
	80·03	·00	·01	·08	·07	·05	·03
Mean	80·03	80·05	80·07	80·07	80·06	80·06	80·08
Probable error ...	·008	·008	·007	·006	·002	·004	·006
Thermometer ...	2	2	2	2	2	2	2

Nitronaphthalin.—A was a sample which had been in my collection for some years, and the details of its preparation are unknown. It was distilled in steam, and crystallized twice from naphtha and thrice &c. (A_3 &c.) from alcohol.

B was a similar sample from the same source. It was purified and fractionated exactly in the same way as A.

Nitronaphthalin becomes sensibly yellowed by continued exposure to light. It melts with considerable sharpness.

Table XXIV.

	A_3.	A_4.	A_5.	A_6.	A_7.	B_4.	B_5.	B_5.
	56°·17	56°·16	56°·17	56°·21	56°·15	56°·24	56°·17	56°·10
	·20	·13	·14	·21	·15	·24	·17	·20
	·14	·21	·12	·16	·18	·19	·20	·15
	·20	·16	·17	·16	·20	·19	·12	·12
	·17	·16	·22	·18	·07	·11	·14	·00
	·20	·21	·22	·26	·15	·16	·09	·15
	·17	·16	·17	·21	·12	·19	·14	·12
	·17	·16	·17	·16	·26	·16	·09	·10
	·17	·16	·14	·13	·14	·19	·09	·10
	·20	·16	·14	·16	·10	·21	·14	·18
	·22	·21	·16	·24	·18	·24	·12	·05
	·21	·21	·22	·16	·23	·16	·20	·15
	·20	·25	·25	·16	·18	·24	·14	·15
	·20	·21	·22	·18	·20	·16	·12	·02
Mean	56·19	56·18	56·18	56·18	56·16	56·19	56·14	56·11
Probable error ...	·004	·006	·007	·006	·009	·007	·006	·010
Thermometer ...	2	2	2	2	2	2	2	3

TABLE XXV.

Summary of Results.

Substance.	Weighted mean.	Probable error.	After Poggendorff's correction.	Air-thermometer.
Toluidine	42°·765	·004	42°·700	42°·890
Nitrophenol (*a*)	44·270	·003	44·205	44·392
Nitrotoluol	51·305	·005	51·239	51·407
Dichlorobenzol	52·723	·002	52·657	52·821
Nitronaphthalin	56·175	·002	56·110	56·261
Dinitrophenol (*a*)	61·778	·003	61·714	61·843
Monobromaniline	61·806	·003	61·742	61·871
Dinitrotoluol (*a*)	69·211	·004	69·154	69·252
" " (*b*)	69·571	·004	69·514	69·610
Monochloraniline	69·667	·003	69·610	69·706
Dinitrobromobenzol	70·598	·004	70·542	70·634
Trichloraniline	77·052	·001	77·004	77·068
Dibromaniline	78·821	·004	78·776	78·833
Trinitrotoluol	78·841	·004	78·796	78·853
Naphthalin	80·061	·002	80·018	80·070
Trinitrotoluol (M)	80·524	·003	80·481	80·532
Nitrodibromobenzol	83·490	·002	83·452	83·492
Dibromobenzol	87·037	·002	87·007	87·035
Dinitrobenzol	89·718	·003	89·693	89·712
Nitrophenol	111·413	·006	111·448	111·455
Dinitrophenol	111·579	·004	111·614	111·621
Tribromaniline	116·247	·005	116·298	116·319
Trinitrophenol	121·082	·005	121·151	121·194

DISCUSSION.

The determinations of melting-point which have been recorded in the preceding tables, and the results of which are summarized in Table XXV., show a very small probable error in connexion with their weighted means. The probable error of a weighted mean has ranged from 0°·001 to 0°·006, its average value being less than 0°·004. So far, then, as regards the actual process of ascertaining melting-point, considerable accuracy has doubtless been attained.

The preliminary operation of calibrating the thermometers was so conducted as not to have introduced material error, as indeed is obvious from a comparison of the results obtained with different thermometers on melting the same substance*. Regnault was of opinion that the height of the barometer cannot be ascertained with a less error than about 0·1 millim. Such an error would correspond to about 0°·0037 on 100°; and the error would be still less on the mean, as in the present case, of several readings. The small errors in the determination of the exposure-corrections could not sensibly affect the

* See, for instance, Table VII.

final results. It is to comparison with the air-thermometer that we have to look for any important source of error. All observers who have made an extended range of such comparisons have found noteworthy errors, though they have in no case stated probable error. The probable error of the result of my own comparisons of thermometer 2 with the air-thermometer is $0^{\circ}{\cdot}085$ for a single set of comparisons, or $0^{\circ}{\cdot}085 \div \sqrt{33} = 0^{\circ}{\cdot}015$ for the results of the thirty-three sets. This number is the measure of probable error of the equation employed in the final reductions. Compounding, then, the mean probable error of the melting-point (·004) with that of the comparison (·015) with the air-thermometer, we may consider the melting-points in Table XXV. ascertained, in terms of the air-thermometer, with a probable error of

$$\sqrt{(\cdot 004)^2 + (\cdot 015)^2} = 0{\cdot}015.$$

The relation of the chemical symbol to the physical properties of a substance is a matter of such great interest that I have sought for it in melting-point, although other investigators of the general subject, working with less definite data, have not arrived at very encouraging results.

It is very easy to show that, in some cases, there is a very simple connexion between the formula and the melting-point of a substance in the centigrade scale. Thus, dichlorobenzol, bromaniline, and trinitrotoluol form a group in which melting-point $= \phi \times$ numerical value of formula.

Substance.	Formula.	Melting-point.	ϕ.
Dichlorobenzol .	$C_6 H_4 Cl_2 = 147$	$52^{\circ}{\cdot}821$	·35933
Bromaniline . .	$C_6 H_6 BrN = 172$	61·742	·35971
Trinitrotoluol .	$C_7 H_5 N_3 O_6 = 227$	80·532	·35477

In the first of these two instances the values of ϕ are almost exactly the same; in the last, however, the limits of probable error are exceeded, though a close approximation is very evident.

The following comparison furnishes another practical identity:—

	Melting-point.			Melting-point.	
Trinitrotoluol .	$78^{\circ}{\cdot}853$	—	Dinitrotoluol .	$69^{\circ}{\cdot}252$	= 9·601
Trinitrophenol .	121·194	—	Dinitrophenol .	111·621	= 9·573

In the next instance there is an approximation:—

	Melting-point.			Melting-point.	
Dinitrotoluol .	$69^{\circ}{\cdot}252$	—	Nitrotoluol .	$51^{\circ}{\cdot}407$	= 17·845
Dinitrophenol .	61·843	—	Nitrophenol .	44·392	= 17·451

Such illustrations may be of service in enabling us to detect, with more or less probability, the parallelism of chemical series, and to enable us to decide whether a function—nitration, for example—has or has not the same value in different parts of a series. Other groups in which a similar but less intimate relation prevails, might be adduced from the list; and a glance at Table XXV. will show that, on the whole, melting-point and formula grow together. It may not improbably prove to be the case that, when the whole subject of melting-point has been successfully investigated, this simple relation is the limiting condition of the real law. The data, however, hitherto adduced are far from adequate to a discussion of numerical relations among melting-points: for such an object it would be a fruitless task to examine them further.

Some negative results of this investigation are worthy of attention. Thus α and β nitrophenol have the same additive formula, and yet differ by 67°·063 in their melting-point. It is clear then, as already well known, that melting-point may, in cases of isomerism, be related to something else than formula. The melting-point of naphthalin is actually lowered in the first stage of nitration. Considerations such as these may perhaps serve as suggestions for future work.

An accurate method of determining melting-point places it within our power to detect far more delicate shades of isomeric differences than have hitherto been regarded as possible. Thus strong presumptive evidence has been adduced (p. 14) that dinitrotoluol, when prepared directly from toluol or from liquid (meta-) nitrotoluol, or from solid (para-) nitrotoluol by gentle nitration, melts at 69°·252; but that when paranitrotoluol is energeticaly nitrated, the product melts at 69°·610. There are consequently two modifications of dinitrotoluol obtainable very directly from toluol—the melting-point of these substances differing by 0°·358, a quantity far beyond the range of error of the method. In like manner, it can be shown that two parallel modifications of trinitrotoluol exist—one of which melts at 78°·853, the other at 80°·532 *.

[For a complete account of the thermometers referred to in this memoir the reader is referred to the Transactions of the Royal Society of Edinburgh, 1881, p. 567; for the method of determining melting-point, to the Proceedings of the Royal Society, vol. xxxiii. p. 203.]

* Compare Phil. Mag. 1875, l. p. 17.

II. *Measurement of Curvature and Refractive Index.* By C. Vernon Boys, *A.R.S.M., Demonstrator of Physics at the Normal School of Science, South Kensington*.*

[Plate I.]

OF the methods best known for measuring the curvature of surfaces, that depending on the spherometer is both accurate and convenient in the case of surfaces of sufficient diameter and where the curvature is not too small. The reflection-test, depending on the observation with a telescope of two images projected on a scale, is certainly not convenient, nor is it capable of giving very accurate results. While endeavouring to find some more satisfactory way of examining the curvature of the surfaces of lenses that would be both accurate and simple, I discovered the method which I am about to describe. Though, from its great simplicity, I can hardly expect it to be new to every one, yet I have never heard of its being employed, nor do those to whom I have shown it remember to have seen it before. Therefore, even if it should be shown that this method is not new, it is certainly so little known that I think it worthy of attention.

The centre-of-curvature test known as Foucault's test, which is used to examine the figure of the mirrors of reflecting telescopes, gives, perhaps, the most delicate means of examining form that exists. By its means the expansion by heat of a portion of the surface produced by touching it with the finger is rendered evident, as an apparent mountain standing out of the glass, which takes from five to fifteen minutes to disappear; and the warm air leaving a hand held between the centre of curvature and the surface has the appearance of flames of fire. In each of these cases no other system of observation could show in so striking and conspicuous a manner effects depending on so slight a cause.

The examination at the centre of curvature is carried out in this way:—The mirror is placed in a convenient support so that its surface is vertical; and in front of it is placed a lamp with an opaque chimney, through which some pin-holes have been made. If one of these holes is near the centre of curvature, the light leaving it and reflected by the surface is brought to a focus on the other side of the centre. This focus is then found; and the lamp is moved till the focus is as near the chimney as will allow of its observation. A piece of thin sheet metal, with a straight edge, is then placed so that it may be moved to or from the mirror or laterally. Now, if the edge of this is sufficiently on one side to let the light

* Communicated by the Author.

pass the focus, an eye immediately behind the focus will see the mirror filled with light; but if it is gradually moved across while the eye still watches the mirror, the illumination of the latter will appear to die away in the same direction as, or in the opposite direction to, the movement of the edge, or uniformly, according as the edge is between the mirror and the focus, or between the eye and the focus, or at the focus. By this means, and by this means only, can the different radii of curvature of the successive zones of a parabolic mirror whose radius of curvature is twenty times its diameter be accurately measured. The close contact between the parabola and the circle is due to the fact that it is one of the third order. I think it worth mentioning that the formula given by Dr. Draper in the 'Smithsonian Contributions to Knowledge,' vol. xiv. (1865), for testing the true parabolic form, gives only half the deviation from the sphere, as was pointed out by a correspondent of the 'English Mechanic' who signs himself "Orderic Vital," and was confirmed by Mr. E. H. Liveing and myself*. I have gone thus fully into the Foucault test, as my method involves the same general principle, viz. making the rays return along the path whence they came.

Before considering the general case applicable to any kind of lens, I think it best first to show the simplicity of the method in a particular and common case—that of a thin equiconvex lens. Fix an ordinary spectacle-lens in a clip, with its principal plane vertical; in front of it place a card with a small hole in it; and illuminate the hole with a candle-flame. It will be found that, when the lens is at a certain distance from the card, there is an inverted image of the hole formed on the card. When this is the case, the light leaving the hole and meeting the front surface of the lens is refracted and meets the back surface normally: most of the light passes through; but a small portion is reflected back along the path whence it came, and is sufficient to produce an image easily visible in the day. This distance of the card from the lens, which is the apparent radius of curvature of the back surface seen through the front surface, is throughout this paper called f. The true focal length F of the same lens may be observed in the usual way; but it is more conveniently found by fixing a plane surface of glass behind the lens, when it will be found that another image may be produced when the lens is about twice as far from the card as it was before. Since an image is produced, the light must have returned along the path whence it came, and must therefore have struck the plane surface nor-

* English Mechanic, vol. xxxi. pp. 89, 184, 207.

mally—that is, have left the lens and returned to it as a parallel beam; therefore the card is at the principal focus. For a plane glass surface a piece of plate glass blacked at the back, or the surface of a prism may be used. The observations of the distances F and f can be easily and accurately made; then the radius of curvature may be found from the formula

$$R = \frac{Ff}{F-f},$$

as I shall presently show.

Before doing so, however, I think it well to describe the most accurate method of observing the distances F and f. The card with the pin-hole is convenient; but it is difficult to find the place with great accuracy where the focus is most sharply defined, and to measure the distance when found. All difficulty is completely avoided by the following plan:—Take a piece of thin sheet metal, of the size and shape shown in Pl. I. fig. 1, and fix in front of it, in the position shown by the dotted line, a small reflecting-prism, so that, when a small bright flame is placed on one side of the prism, a beam of light leaves the slit in the plate. Replace the card by this plate and prism, and move the lens till the aerial image of the slit is formed in the corner, close by the edge of the prism. To examine the position of the image with greater precision, an ordinary positive eyepiece will be found convenient. When the image and the slit are equally distant from the lens, there will be no relative movement on moving the eye; if there is relative movement, the distance between the lens and the plate must be increased or diminished according as the plate or the image appears to move with the eye. When the distance has been properly adjusted, it is easily measured by resting a scale on the continuation of the lower edge of the slit, and moving it till it touches the surface of the lens. The position of the edge of the prism or of the slit may then be read with great accuracy; and it will be found that, on repeating the observations several times, a discrepancy more than a tenth of a millimetre between any of the measures need not occur.

Instead of observing the position with an eyepiece, the Foucault plan may be adopted. Place the eye immediately behind the edge of the prism, so that all the light forming the image enters the eye. Move the prism laterally towards the image, which of course moves to meet it, and observe whether the light which fills the lens dies away uniformly, or whether it seems to retreat from one edge of the lens. If the retreat is in the same direction as the movement of the prism-plate, the distance is too small; if in the opposite direction, too

great. Either of these systems will give accurate results; I prefer the first, as tiring the eye less and being, especially with small lenses, the more accurate.

A convenient support for the lens is made by boring a hole, with a less diameter than the lens, in a piece of thin parallel-sided wood. The lens may be slipped under two clips, so as to rest against the edge of the hole on one side of the wood. On the other side a piece of plate glass, blacked at the back, is cemented or held in a similar way by clips. If this piece of wood is fixed vertically on a horizontal slide, it may be moved away from the prism-plate, and the distances f and F determined in a few minutes. Fig. 2 is a horizontal section of the arrangement when the principal focus F is being determined. The dotted line shows the position for f.

If instead of a lens a single surface only is to be measured, there is of course no difficulty in the case of a concave surface; but a convex surface may have its curvature determined in the following way:—Arrange the prism-plate and flame as before. At a distance in front of the prism-plate more than its focal length fix a converging lens, preferably achromatic. Observe the position of the aerial image on the other side of the lens, and make it coincident with the edge of a plate of metal, m. The positions must be so adjusted that the distance of m from the lens is greater than the radius of curvature of the given surface. Now place this surface between the metal plate and the lens, and move it till an image is formed accurately by the side of the prism. Then the light impinging on the convex surface has been reflected back along the path whence it came, and has therefore struck that surface normally; therefore the place m, where those rays would have met had they not been intercepted, is the centre of curvature of the convex surface. Its radius of curvature can therefore be measured by suitably-formed callipers. Fig. 3 is a horizontal section of the arrangement.

I have stated above that $R=\frac{Ff}{F-f}$, in the case of a thin equi-convex lens. This must now be proved, and the more general case of any kind of lens treated next. First, consider that the lens is so thin that any normal to either surface cuts each at points appreciably equally distant from the axis. Since the image which is produced is partly formed of rays which are near the axis, these rays meet the axis at angles so small that the tangent, the sine, and the arc are convertible terms. If the lens is large and not of very long focus, this will not be true of rays from near the edge of the lens; but as these rays are not necessary for the image, the central ones alone may be

employed, and by them the curvature of the whole surface, if spherical, determined.

On the front surface take any point p, and through it draw a radius mR of the back surface. Join p with f, the apparent centre of curvature of the back as seen through the front surface. Draw also through p a radius ab of the front surface and a line dc parallel to the axis. Then the angles mpd, dpb, apc are equal to one another. Call these angles θ. The angle $apf = \mu \times$ angle $mpb = \mu 2\theta$; therefore the angle $cpf = \mu 2\theta - \theta$. But the angle $ep\mathrm{R} = \theta$;

$$\therefore \qquad \frac{\mathrm{R}}{f} = 2\mu - 1, \text{ or } \mu = \frac{\mathrm{R}+f}{2f}.$$

By the property of equiconvex lenses, $\mu = \frac{\mathrm{R}}{2\mathrm{F}} + 1$;

$$\therefore \qquad \frac{\mathrm{R}}{2\mathrm{F}} = \frac{\mathrm{R}-f}{2f} \text{ and } \mathrm{R} = \frac{\mathrm{F}f}{\mathrm{F}-f};$$

or, in a thin equiconvex lens, the radius is equal to the product divided by the difference of the principal focal length and the apparent radius of the back as seen through the front surface.

It might be expected that, as this formula has been deduced from a specially simple case, a more complicated one would be necessary if the two sides of the lens were not equally curved, or if one surface were plane or concave. But such is not the case; the same formula applies in every possible case, though, as will be shown, experimental difficulty occurs in the case of a diverging meniscus.

The proof of the formula in the case of a thin lens which is not equiconvex is similar to that already given. Make the same construction as before, and let R_1, R_2 be the centres of the surfaces 1 and 2, and f_1 the apparent centre of 1 seen through 2.. Also let $\mathrm{R}_1 = n\mathrm{R}_2$. Call each of the angles mpd, $cp\mathrm{R}_1$, θ; then the angles $dp\mathrm{R}_2$ and apc will each equal $n\theta$. As the angle $apf_1 = \mu \times$ angle $mp\mathrm{R}_2 = \mu(1+n)\theta$, $\therefore$ angle $cpf_1 = \mu(1+n)\theta - n\theta$. But angle $cp\mathrm{R}_1 = \theta$;

$$\therefore \qquad \frac{\mathrm{R}_1}{f_1} = \mu(n+1) - n.$$

Substitute $\frac{\mathrm{R}_1}{\mathrm{R}_2}$ for n, and it will be found that

$$\mu = \frac{\mathrm{R}_1(f_1+\mathrm{R}_2)}{f_1(\mathrm{R}_1+\mathrm{R}_2)}. \qquad \ldots \ldots \qquad (1)$$

A similar proof will show that

$$\mu=\frac{R_2(f_2+R_1)}{f_2(R_1+R_2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

By the property of lenses,

$$\mu=\frac{R_1R_2}{F(R_1+R_2)}+1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where F is the principal focal length.

By combining (1) and (3),

$$\frac{1}{R_1}=\frac{1}{f_1}-\frac{1}{F}, \text{ or } R_1=\frac{Ff_1}{F-f_1}. \quad . \quad . \quad . \quad . \quad (4)$$

By combining (2) and (3),

$$\frac{1}{R_2}=\frac{1}{f_2}-\frac{1}{F}, \text{ or } R_2=\frac{Ff_2}{F-f_2}. \quad . \quad . \quad . \quad . \quad (5)$$

By combining (1) and (2),

$$\frac{1}{R_1}-\frac{1}{R_2}=\frac{1}{f_1}-\frac{1}{f_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It is not a little surprising that, whatever the refractive index of the material of the glass, or the curvature of the front surface, the curvature of the back surface can always be obtained from an expression in which both apparently are omitted. They are both of course involved in each observation, F and f, which accounts for the possibility of their being eliminated.

It is interesting to follow the changes which occur between the two extreme limits of form—a double convex and a double concave lens. Take a double convex lens, and suppose one of the surfaces to be gradually pushed in; when it has become plane we have the first particular case—a plano-convex lens. Call the flat surface 1 and the convex surface 2; then, by (4),

$$\frac{1}{\infty}=\frac{1}{f_1}-\frac{1}{F};$$

$\therefore f_1=F$, or the apparent centre of curvature of the flat surface seen through the round surface is at the principal focus. By (5),

$$\frac{1}{R_2}=\frac{1}{f_2}-\frac{\mu-1}{R_2},$$

since $F=\frac{R_2}{\mu-1}$; $\therefore R_2=\mu f_2$, or the apparent radius of 2 is less than the true radius in the ratio of μ to 1.

If the pushing-in process is continued, the surface 1 will become concave. Four observations can then be made—F, f_1, f_2, and R_1; therefore R_2 may be found by either of the

equations (5) or (6). As the surface 1 becomes more concave, its apparent centre on the other side of the lens will retreat to an infinite distance; and then the concave side will appear flat when viewed through the convex surface. This is the case when $R_1 = -F$, as may be shown by making $f_1 = \infty$ in (4), or as is obvious from a diagram. When R_1 becomes less than this, f_1 becomes an imaginary point on the other side of the lens, such that, if rays were sent so as to converge upon it, they would return as though they had come from it. Its position could be determined experimentally by the method given for a convex surface on p. 33; but as the true radius can be determined directly, there is no necessity to find this imaginary apparent radius.

Let the concavity of surface 1 increase; the next particular case is that of a watch-glass, where $R_1 = -R_2$. Then F becomes infinite, and the two points f_2 and R_1 become coincident. When the surface 1 becomes still more concave, F becomes negative and virtual, and R_1 and f_2 pass one another. The experimental determination now becomes more difficult; for neither can F or f_1 be observed directly; but still the equations (5) and (6) hold. They may each be found by the method for a convex surface, which is less convenient than the direct method.

If the concavity of the surface 1 continues to increase, another limit will be reached, at which f_2 becomes infinite. This is obviously the case when $R_2 = -F$; that is, when the focal length has been so shortened by the increasing concavity as to be equal to the radius of the convex surface. When this is the case, the surface 1 seen through 2 appears plane. When the concavity passes this limit, f_2 becomes negative and imaginary, and the experimental difficulty is still further increased, for R_1 only can be directly observed; but still the equations (4), (5), and (6) are true. No further increase in the concavity of 1 will produce any new conditions. Now, the curvature of 1 remaining constant, let 2 become flatter; when it has become plane, there is no occasion to observe F, f_1, or f_2 to determine the form of the surfaces. When 2 becomes concave also, the curvature of each surface can be directly measured; and all difficulty is removed. Every possible case has now been considered; and though the equations are always true, experimental difficulty only occurs in the two classes of diverging meniscus.

If a parallel beam of light falls on the lens, it will be refracted at the front surface, partly reflected from the back, and again refracted at the back surface, and be brought to a focus at a distance from the lens equal to half the apparent radius.

As the light, in its passage to and from the apparent centre f_1, is twice refracted by 2 and once reflected by 1, it would seem at first sight that the value of f_1 might be obtained by combining in the usual way the expression for twice the focal length of an equiconvex lens with surfaces having the same curvature as 2 and the radius of 1; thus

$$\frac{1}{f_1}=\frac{1}{2F}+\frac{1}{R_1}.$$

But this operation, depending on a false assumption, leads to an erroneous result. It makes $f_1=\frac{R_1R_2}{\overline{\mu-1}R_1+R_2}$, instead of $\frac{R_1R_2}{\overline{\mu-1}R_1+\mu R_2}$. The error arises in this way:—When a double convex lens is employed, either to bring or to hurry light to a focus, the bending-powers of the two surfaces depend on the angles they make with the ray in the lens. Now, if one of these angles is great, the other must be small; so that, as a combination, they have the same focus-shortening power, however the light falls on them. But when a ray passing from and returning to the apparent centre f strikes the front surface, that surface makes an angle with the ray in the lens which is greater than the mean in the ordinary way; therefore the surface produces a greater diverting effect; and hence the distance f is less than it would be if the supposition made were correct.

All that has been shown at present is only true when the thickness of the lens is inappreciable. When this is not the case, rays, whether from the principal focus F or from an apparent centre f, will not cut the two surfaces at points equidistant from the axis. First, consider the case of an equiconvex lens. Let fig. 5 represent a portion of a thick equiconvex lens. As before, since the central rays are sufficient to give an image, arcs, sines, and tangents may be considered identical. On one surface take any point p. Through it draw a radius pR, and the line pbc parallel to the axis. Now a ray of light parallel to the axis, meeting the surface in the point p with an angle of incidence equal to θ, will be refracted so that the angle dpe is equal to $\frac{\theta}{\mu}$; therefore the angle $bpd=\theta\frac{\mu-1}{\mu}$. Therefore the line pd continued will meet the axis in a point a such that

$$pa=\frac{\mu R}{\mu-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But this ray is diverted at the point d, and bent down so as to

meet the axis at the principal focus F. It is required to find the length dF, or, shortly, F—that is, the distance of the principle focus from the surface. Since the line pa has cut the front surface at a point d nearer the axis than p, the inclination of the normal, bs, at d will be less than θ, and will equal $m\theta$ if

$$m=\frac{pa-t}{pa}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

t being the thickness of the lens. Now the angle pds = the angle dbp + the angle $bpd=\theta\left(m+\frac{\mu-1}{\mu}\right)$; therefore the angle $bd\mathrm{F}=\mu\theta\left(m+\frac{\mu-1}{\mu}\right)$, and the angle

$$gd\mathrm{F}=\mu\theta\left(m+\frac{\mu-1}{\mu}\right)-m\theta=\theta(\overline{1+m}\,\overline{\mu-1}). \quad . \quad . \quad (3)$$

Since the lines dF and da leave the same point d with different inclinations, they will meet the axis at distances which are inversely as these angles. Therefore

$$\frac{da}{\mathrm{F}}=\frac{\overline{1+m}\,\overline{\mu-1}}{\frac{\mu-1}{\mu}}=\mu\overline{1+m};$$

therefore

$$\mathrm{F}=\frac{pa-t}{\mu(1+m)}=\frac{\mathrm{R}\left(\frac{\mu\mathrm{R}}{\mu-1}-t\right)}{2\mu\mathrm{R}-t(\mu-1)}, \quad . \quad . \quad . \quad . \quad (4)$$

by (1) and (2). This is the distance of the principal focus from the surface of the lens.

The distance ef of the apparent centre from the surface may be found in a similar way. The normal at e makes an angle $n\theta$ less than θ, such that

$$n=\frac{\mathrm{R}-t}{\mathrm{R}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The angle $pet=(n+1)\theta$; the angle $cef=\mu(n+1)\theta$; and the angle $kef=\theta[\mu(n+1)-n]$. Therefore

$$\frac{ef}{\mathrm{R}-t}=\frac{1}{\mu(n+1)-n}, \text{ and } ef=\frac{\mathrm{R}\overline{\mathrm{R}-t}}{(\mu-1)(\mathrm{R}-t)+\mu\mathrm{R}}. \quad . \quad (6)$$

This is the distance of the apparent centre from the surface. By (6),

$$\mu=\frac{(\mathrm{R}+f)(\mathrm{R}-t)}{f(2\mathrm{R}-t)}.$$

Substitute this value of μ in (4); on simplifying, it will be

found that

$$R^2 - R\left(\frac{f^2}{F-f} + 2t\right) = \frac{Ff^2}{F-f} - t^2, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

from which R may be found if F, f, and t are given. If t is made equal to 0, equation (7) gives

$$R = \frac{Ff}{F-f}, \text{ or } -f.$$

The first result is the same as that already found for a thin lens; while the value $-f$ seems to have no physical meaning.

If the thick lens is not equiconvex, there are five observations possible—the distances of the two apparent centres from the surfaces, the distances of the two principle foci from the surfaces, and the thickness; but there are only three things to be determined—the two real radii and the refractive index: therefore the equations for R_1, R_2, and μ must be capable of solution. The following are the expressions which may be found by a similar treatment of fig. 5 to that already employed in the case of the equiconvex lens, if it be remembered that all the angles made by surface 1 are $\frac{R_2}{R_1}$ times those made by surface 2 at the same distance from the axis. They are

$$F_1 = R_2 \frac{\frac{\mu}{\mu-1}R_1 - t}{\mu(R_1+R_2-t)+t}, \qquad F_2 = R_1 \frac{\frac{\mu}{\mu-1}R_2 - t}{\mu(R_1+R_2-t)+t},$$

$$f_1 = R_2 \frac{R_1 - t}{\mu(R_1+R_2-t)-(R_1-t)}, \quad f_2 = R_1 \frac{R_2 - t}{\mu(R_1+R_2-t)-(R_2-t)}.$$

The first two of these equations give

$$\frac{F_1}{F_2} = \frac{\mu - R_1 t(\mu-1)}{\mu - R_2 t(\mu-1)};$$

and the second two give

$$\mu = \frac{\overline{R_2+f_1}\,\overline{R_1-t}}{f_1(R_1+R_2-t)} \quad \text{and} \quad \mu = \frac{\overline{R_1+f_2}\,\overline{R_2-t}}{f_2(R_1+R_2-t)}.$$

By these μ may be eliminated. The solution for R_1 and R_2 I have not obtained; but I do not think there is any difficulty.

The following application to the case of liquids of the principle of making the rays return along the path whence they came, forms a neat though impracticable method of determining their refractive index when greater than $\sqrt{2}$:—Replace the cross-wires of a telescope by a prism-plate, as already described, but in which the slit is longer and adjustable. Fix opposite the object-glass a piece of parallel-sided plate-glass, with its plane at right angles to the optical axis. The cor-

rectness of this adjustment may obviously be determined at the eyepiece. Let the glass plate form the bottom of a trough in which the liquid may be placed, and let there be under this a dish containing mercury. Now, there is a certain inclination of the telescope at which the beam from the illuminated slit, rendered parallel by the object-glass, is refracted at the free surface of the liquid, and again at the surfaces of the glass plate, so as to leave the latter vertically; then, striking the mercury, it returns along the path whence it came, and may be viewed by the eyepiece. Under these conditions the beam of light on either side of the prism is at right angles to the opposite side; therefore they make equal angles with the adjacent sides, and the prism is at minimum deviation. Calling the inclination of the telescope θ, we have $\sin\theta = \mu \sin\frac{\theta}{2}$; therefore $\mu = \sqrt{2(1+\cos\theta)}$. The telescope may be first inclined on one side and then on the other, and half the angle moved over taken as θ. I have found it utterly impossible to get the sodium-line to keep still for a moment, or to be even fairly defined, as every movement in the neighbourhood produces a tremor on the surface of the liquids, which, in the Science Schools at Kensington at any rate, is so continuous as to make it impossible to observe with accuracy. I should have said that the top of the prism must be covered in with a glass plate, to prevent the evaporation and consequent superficial cooling of the liquid forming the prism, which causes striæ in the liquid, spoiling definition even more than the continuous tremor.

Helmholtz and others have shown that, during accommodation of the eye for near objects, the cornea does not change in curvature, the front surface of the lens becomes more curved and advances, and the back surface does not appreciably change. The proof given is that the images of a light produced by reflection from the cornea and from the back surface of the lens do not change, while that produced by the front surface of the lens advances and becomes smaller. Now it would appear at first sight that these observations prove a flattening of the back surface of the lens during accommodation; for if it did not change in curvature, the rays of light passing twice through the more curved front surface would sooner come to a focus; but since they do not apparently sooner come to a focus, it would seem that a flattening of the back surface must have occurred to counteract the shortening influence of the more curved front surface. In the case of ordinary lenses this would be so; but it so happens that in the crystalline lens the focus by reflection is formed

within it, and so the more curved front surface magnifies the smaller image, which therefore appears unchanged.

While on the subject of reflection in lenses, I think it worth while to mention that convex lenses silvered at the back make excellent and easily constructed concave mirrors. Since both surfaces conduce to bring light to a focus, flatter curves may be used than are necessary for a plain concave reflector of the same focal length; also, since the two surfaces are not parallel, false images are not produced; so that the advantage of glass silvered at the back remains, without the usual disadvantage. A spectacle-lens of about five inches focal length, silvered at the back and mounted, forms an eye-glass (I mean a glass for examining the eye) which every one who works in metal should possess. I have found by its means specks of metal, thrown from the lathe, which were utterly invisible by other means, but which were nevertheless exceedingly painful.

III. *Experiments on the Faure Accumulator.*

By Professors W. E. Ayrton *and* John Perry*.

Having made, at the request of the Faure Accumulator Company, a series of experiments on some of their cells, we have thought that a short account of some of the results obtained may not be uninteresting to the members of the Physical Society.

The object of the experiments was to ascertain, *first*, the efficiency of a cell—that is, the ratio of the energy given out by it to the energy put into it; *secondly*, the storing-power of a cell; and, *lastly*, whether or not there was a deterioration in its working-powers. To measure the energy put into any electric circuit, we have merely, of course, to take time-readings of the current flowing through the circuit, as well as the difference of potentials between its two extremities. The current in ampères multiplied by the electromotive force in volts and by 44·25, gives the number of foot-pounds per minute that is being put into that part of the circuit as electric energy. For measuring the current we have used throughout our ammeters (short for ampère-meters), and for measuring electromotive force our voltmeters, the latter being employed of course in a shunt circuit.

Of the total electric energy put into the circuit, and which is measured, in foot-pounds per minute, by 44·25 AV, a portion will be employed simply in heating the circuit, and the

* Communicated by the Physical Society, having been read at the Meeting on February 25, 1882.

remainder may be utilized in producing useful work. For example, if a time-curve be drawn for 44·25 AV when charging a Faure accumulator, the area of the curve will measure the total energy put into the accumulator in foot-pounds; but of this some portion has been wasted in heating the cell, due to the charging having been more rapid than was absolutely necessary. It was, of course, of considerable importance in our experiments to ascertain what portion of the energy put into the cell was really thus wasted; and to measure this the following experiments were made.

Occasionally the main current was stopped, the shunt current through the voltmeter being left completed. The reading now on the voltmeter gives the difference of potentials produced by the cell itself, whereas the previous reading was the combined difference of potentials produced by the cell and the dynamo-machine charging it. If now a new time-curve be drawn in which the ordinates represent the product of 44·25 AV′, where V′ is the electromotive force of the cell measured on the circuit being broken, and A is the mean value of the current flowing just before breaking and just after closing the circuit, the area of the new curve will represent that portion of the energy put into the cell which is usefully employed in chemical decomposition. The difference between the areas of these two curves represents, then, the amount of energy wasted in heating the cell in foot-pounds.

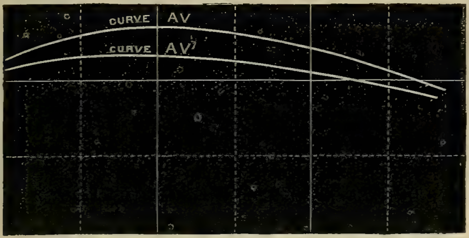
Again, on discharging the cell, experiments of a similar nature have to be made. The product 44·25 AV represents the number of foot-pounds of work per minute the cell is producing in the external circuit, V being the difference of potentials between the two poles of the cell while it is discharging; but, in addition, there is a certain amount of energy which is being expended in heating the cell itself during discharge.

This, as before, may be ascertained by breaking the main circuit, leaving the shunt-voltmeter circuit completed. The reading on the voltmeter V′ now indicates the real electromotive force of the accumulator during discharge; whereas the previous reading, obtained just before breaking the circuit, represents merely the fraction of the total electromotive force employed in sending the current through the external resistance. If a time-curve be drawn with its ordinates proportional to 44·25 AV′, where A is the mean value of the current just before breaking and just after closing the circuit, its area will represent the total number of foot-pounds of energy per minute being given out by the cell; and the difference between the areas of the last two curves will represent the number of foot-pounds of energy employed in heating the cell itself. It

is to be noticed that during charging V′ is less than V, whereas on discharging V′ is greater than V.

An examination of thirty-five sheets of time-curves, which we have drawn from the experiments we made, shows that, in charging, the curve for AV rises at first; and as it rises more rapidly than that for AV′, this means an increase in the resistance of the accumulator.

E.M.F. × Current.



Time.

As the charging continues, the two curves for AV and AV′ approach one another, showing that the internal resistance of the accumulator diminishes again. On the other hand, at the end of a long discharge the curve for AV falls more rapidly than that for AV′, due to an increase in the internal resistance. Now our experiments show a great constancy in the electromotive force of a Faure cell, and that the falling-off in discharging which occurs during a very rapid discharge, or at the end of a long discharge, is due more to an increase in the internal resistance of the accumulator than to a diminution in the electromotive force, which our methods of experimenting above described enable us to separate and measure independently. But, whether discharging rapidly or whether discharging slowly, there is a most curious resuscitating-power in the cell, which, if disregarded, will cause totally erroneous underestimates to be made of the efficiency of the cell.

This resuscitating-power is more marked for rapid discharges than for slower ones. In the case, for example, of an extremely rapid discharge, we found that when the flow had become apparently so feeble that the cell appeared totally discharged, leaving the poles of the cells insulated caused three times as much electric energy to be given out all together in the second discharge as had been given out in the first. And even when several days are taken to discharge the cell—and we may mention that we have had continuous observations made day and night for several days in certain cases—this

resuscitating-power is wonderfully marked. An insulation of a few hours will cause the energy given off per minute on redischarging to be eight to ten times as great as it was before insulation. Indeed on one occasion, after a cell had apparently nearly discharged itself, it was left shortcircuited with a thick wire for half an hour, then insulated all night, when the number of foot-pounds of work per minute given off at the commencement of the discharge the following morning was found to be ten times as great as it was on the previous evening, and a greater amount of energy was actually taken from it in the second discharge than in the first. This phenomenon gives the Faure accumulators a great value for tramcar propulsion, since, as is well known, it is just on starting after stopping that the strain on the horses is so great.

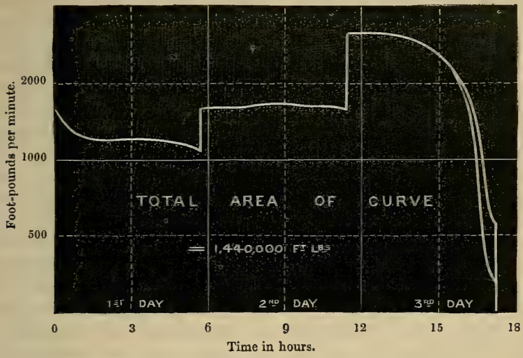
Efficiency.—To determine the efficiency of cells we commence with them empty, or at least as empty as many hours' shortcircuiting with a thick wire could make them. We then measured the total amount of energy put in and the total amount subsequently given out, and we found that, for charges up to a million foot-pounds put into the cell and discharged with an average current of 17 ampères, the loss in charging and discharging combined may not exceed 18 per cent. Indeed, for very slow discharges the loss in charging and discharging combined in some of our experiments has been as low as 10 per cent.

Storing-power.—It is a little difficult to measure the maximum storing-capacity of the cell at the same time that measurements are made of its efficiency, because in the latter case we must take care that we do not put in more electric energy than the cell can hold; on the other hand, if precautions are taken to avoid overcharging, it is a little difficult to ensure that the full charge has been put in. We have therefore separated our experiments for measuring the efficiency from those employed to ascertain the storing-power.

Let us take a single example of the storing-capacity. A certain cell containing 81 lb. of lead and red lead was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot-pounds of work. This is equivalent to one horse-power for three quarters of an hour, or 18,000 foot-pounds of work stored per pound weight of lead and red lead. The curve shows graphically the results of the discharge.

Horizontal distances represent time in minutes, and vertical distances foot-pounds per minute of energy given out by the cell, and the area of the curve therefore the total work given

out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on



the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging with a current of about 25 ampères. And this cell, like the others, showed, on being insulated after having been apparently totally discharged, that there was still a large charge stored up; hence the numbers given above for the capacity are probably under the total value.

Deterioration.—As to deterioration, two months constant charging and discharging of the two accumulators under test showed no signs of deterioration.

IV. *A Simplified Dispersion-Photometer.*
By Professors W. E. Ayrton *and* John Perry*.

IT will be in the recollection of the Members that in 1879 we described to the Society a dispersion-photometer which enabled measurements to be made of the intensity of the strongest electric light in a small room and for the rays coming from the electric light at any angle—two essentials which appeared to us necessary in an electric-light photometer. The principle of this photometer consisted in our use of a concave lens to weaken the strength of the light, so as to make the illumination of a screen comparable with the illumination of a standard candle, instead of keeping the lamp a distance of

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50 or 100 feet away, which was the plan in use until that time. We exhibit now five successive forms of the instrument, which illustrate the history of its development to the present time.

1. The first of these is very nearly the same as that described in our former paper, with the exception that we discarded the use of a long screw (shown in our original figure) for adjusting the position of the lens—as we found that a very easy adjustment might be effected with the fingers, the tension of the bellows part making an automatic clutch which fixed the lens-slide in any position.

2. The second specimen is on the same principle, only that telescope-tubes are used instead of a wooden frame and a bellows. Instead of the lens part alone tilting when the elevated or depressed light has to be examined, the candle-box is here made to tilt also, the candle being supported in gimbals so that it may remain vertical for every angle of elevation.

3. The third specimen is on pretty much the same principle; but as we found a difficulty in comparing two illuminated disks whose centres were some distance apart, we arranged in front of these disks two mirrors, which enable us to make the comparison between two illuminated semicircles having the same diameter. The difficulty of adjusting the lens and making a comparison of the illuminations, and reading the scale, without moving one's head, in all these early instruments led us to the

4th form, which is probably familiar to the Members, as it was exhibited at Paris and largely used there for measurements. In this the candle-box and the lens-box are placed end to end, the lens is fixed in a wooden piston which moves in its hollow square box, which is lined with velvet; and the lens shows its position by a pointer moving over the scale outside. The pointer projects from the inside of the wooden cylinder at any point of a long slot, whose sides are made of india-rubber tubing, so that no extraneous light can reach the illuminated screen. A little handle working a rack and pinion enables the lens to be placed in any position. Through a hole at the side the two screens can be viewed reflected in two mirrors, inclined to one another in the space between the candle-box and the lens-cylinder; and the illuminated papers are viewed as two semicircles having a common diameter. In front of this hole we have slides of red and green glass; so that, as our custom has always been, we make two measurements—one a comparison of the ruby-red light of the lamp examined with the red light of the candle, and another of the green lights. This instrument differed from the earlier forms in not requiring any calculation to be made of the strength of the

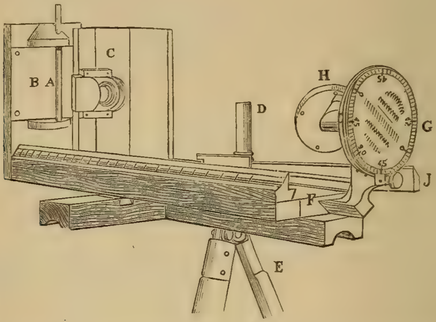
light; that is, the reading of the pointer was not merely a reading of its distance from the screen, but it was a reading in standard candles of the power of the light. Three such scales were placed on the instrument; and there were three certain distances at which the lamp had to be placed for examination. The tilting-arrangement was of course different from that of the earlier forms.

As the instrument had by this time (the end of last year) come into a rather extensive practical use, we had opportunities of seeing that, as an instrument to be used by unscientific persons, it was not yet in a perfect condition, in spite of the many changes that had been made in its construction. The most important difficulty was due to the fact that a slight lateral change in the position of the observer's eye caused the apparent illumination of the screens to vary. Being aware of this fact ourselves, we maintained a certain fixed position of the eye when making observations; but the instrument could not at once be used by persons not accustomed to make delicate experiments.

5. The fifth form, which we now present to the Society, is the outcome of our labours on this subject. We have all along seen the disadvantage of using the Bouguer's two-screen method, since, when lights are examined that have passed through tissue or tracing-paper, a very slight change in the position of the observer's eye makes a very great difference in the apparent illumination, whereas, using Rumford's method, when a sheet of white blotting-paper is employed as a screen very considerable changes in the position of the eye produce no change in the apparent illumination—a result, however, which is not attainable when ordinary drawing-paper is used as the screen. If, however, Rumford's method is to be used to measure the rays coming at different angles from an electric light, a mirror must be employed to reflect them successively onto the same screen; and if used in the ordinary way, the angle of incidence of the rays on the mirror will be different in different cases. Now the difficulty that always met us arose from the inequality of the reflecting-power of an ordinary mirror for rays falling on it at different angles of incidence. We have, however, completely overcome this difficulty in an extremely simple way, by causing the mirror to turn about a horizontal axis inclined at 45° to its plane, and the whole photometer to turn about a vertical axis. With this arrangement the angle of incidence, and consequently the proportional absorption, is the same whatever be the inclination of the rays coming from the lamp to the mirror; and, further, the angle being 45°, the amount of rotation of the

mirror about its horizontal axis necessary to enable measurements to be made of rays coming at any angle, after measurements have been made of the horizontal beam, is exactly equal to the inclination of the beam in question.

Using Rumford's method in this latest form of our photometer, we are to a great extent independent of the presence of other sources of illumination of the screen, so that the apparatus need not be enclosed in a box. At the same time, however, the sensibility of the test is much increased by placing a shade to prevent the electric light shining directly onto the screen. On this screen of blotting-paper, B, is thrown the shadow of a black rod, A, placed in front of it, by a candle in the candle-holder, D.



Now it is well known that if an electric light is also allowed to illuminate this screen, and to throw a second shadow of the rod A on the paper, and if the candle is adjusted at such a distance that the two shadows are of equal intensity, the strength of the light is to that of the candle in the ratio of the squares of their distances from their respective shadows. But instead of allowing the strong light to pass directly to the screen, we cause it to pass through the concave lens in the sliding wooden frame C. A pointer on this slide tells the distance of the lens from the screen. As you are all aware, the weakening of light-intensity produced by the lens enables us to leave our electric lamp within a few feet of the instrument. We have experimentally found that there is no appreciable loss of light in passing through the lens. The candle slides on the bar J; and its distance from its shadow is shown by a pointer on a scale. If f is the focal length of the lens, D the distance of the electric light from the paper-screen, d

the distance of the centre of the lens from the screen, and c that of the candle when the shadows show equal illumination, then, if L is the strength of the examined source of light in standard candles,

$$\sqrt{\mathrm{L}} = \frac{\mathrm{D}-d}{c}\left\{1+d\left(\frac{1}{f}+\frac{1}{\mathrm{D}-d}\right)\right\},$$

or

$$\mathrm{L} = \frac{1}{c^2}\left\{\mathrm{D}+\frac{d(\mathrm{D}-d)}{f}\right\}^2.$$

For our own use we prefer to employ the formula; but as all the common instruments which have hitherto been manufactured have lenses whose focal length is 4 inches, we have prepared a table, a copy of which is sent out along with each instrument, in which the value of L is given for various values of D, d, and c. Using this table, it is necessary to have the lamp at either 60, 120, or 300 inches from the screen; the candle is either at 10, 14·14, or 20 inches from the screen; and the table is made out for every half inch of the lens-scale. But inasmuch as we find that the improved arrangement of the mirror already referred to constitutes perhaps the most useful part of the instrument, and as the use of this improvement involves many alterations of D, the manufacturer proposes in future not to furnish any table of values of L unless specially asked for.

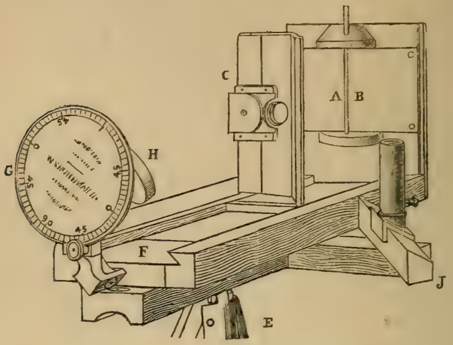
H is the plane silvered-glass mirror which makes the angle of 45° with the axis of the lens, and with the axis about which the mirror itself is free to revolve. As already explained, a ray of light reflected from the mirror and passing through the centre of the lens must, for any position of the mirror, have an angle of incidence of 45°, and so must experience the same amount of absorption, from whatever direction it may have come to the mirror. Further, this angle being 45°, a fixed pointer marks on the graduated circle G, which moves with the mirror, the angle which any ray we may be examining makes with the horizontal.

In this instrument we find that from 30 to 34 per cent. of the incident light at 45° is absorbed, whether this light is of ruby-red or signal-green colour; so that we have the easy practical rule for all cases—add one half to the measured intensity of light reflected.

We need not here refer to the fact that, when investigating the efficiency of an electric lamp, we always measure the horse-power given electrically to the lamp simultaneously with the photometric measurement.

The lamp is suspended in such a way that it can readily be

placed at any elevation. The frame of the tripod-stand is first levelled. A pin at F, directly underneath the centre of the



mirror, passes through the base of the photometer and fits into a hole in the top of the tripod-stand. The photometer, by turning round this pin, can, without producing any change in the distance of the centre of the mirror from the lamp, and therefore without changing the distance from the screen to the lamp, receive the small horizontal motion necessary for the adjustment of a new inclination of the rays coming from the electric light, without any alteration of the distance of the centre of the mirror from the lamp. The divided circle is clamped with the index at 0°; the lamp is lowered or raised till the illuminated disk formed by the reflected light, passing afterwards through the lens, is in the middle of the paper screen. A little sliding shutter with a fine hole in its centre, seen in the figure, enables a very exact adjustment to be made; but in practice we find that we get sufficient accuracy without the use of the shutter. We now measure the distance from lamp to centre of mirror in inches. Equalizing the intensities of the two shadows by adjusting the lens-slide when looking at them through red or green glass, we now note the lens- and candle-readings ; and we repeat these operations, changing from red to green and green to red about five times in a minute. The lamp is now raised or lowered and fixed in any position ; a few seconds suffice to turn the mirror so that it sends its centre ray exactly through the centre of the lens. The distance from screen to mirror in this instrument being 22 inches, if δ is the distance from centre of mirror to vertical from lamp.

and if θ is the angle of elevation, then

$$D=22+\delta \sec \theta.$$

Using this value of D in the formula above, and adding one half to the strength of the light to make up for absorption, the true intensity of the light in standard candles can be ascertained. We find in practice that, if an electric light is moderately steady, ten measurements may be made, with some confidence in their accuracy, in two minutes; and the light may be measured in ten different positions, from an angle of depression of 60° to an angle of elevation of 60°, 100 observations being taken, in less than half an hour.

We may mention one very important result we have been led to by the systematic employment of a photometer which can be used close to the electric light; and that is the large amount of absorption that occurs on certain days when the rays from strong electric lights, and especially the green rays, pass through the air which appears to the eye perfectly clear. At first we were inclined to think the higher results for the candle-power of a lamp obtained with our dispersion-photometer than those obtained with an ordinary distance-photometer were due to some error in our photometer itself; but we have since ascertained that this is due to the absorption of the air—because we find that, if simultaneous measurements are made with ordinary Rumford's photometers, each without lens or mirror, placed at different distances from the lamp in the same azimuth and in the same horizontal plane, the nearer one gives, as a rule, the highest readings; and the difference is the greater the stronger the light, and is greater if the light be examined at each photometer with green glass.

V. *On the Connexion between Viscosity and Density in Fluids, especially Gaseous Fluids.* *By* E. Warburg *and* L. v. Babo *.

THE laws according to which the elasticity and viscosity of a body are connected with its density are of great simplicity in the case of gaseous bodies. The elasticity of these, *i. e.* the reciprocal of their compressibility, is given, according to Boyle and Mariotte's law, by the pressure, and is proportional to the density; the viscosity, measured by the coefficient of friction, is, according to Maxwell's law, independent of the density.

It is known that the first of these laws, that which refers to

* Translated from the *Sitzungsberichte der K. Preuss. Akademie der Wissenschaften zu Berlin*, May 4, 1882, pp. 509–514.

elasticity, holds only approximately, and even that only at moderate degrees of density; at higher densities, according to the investigations of Natterer, Andrews, Cailletet, and others, the connexion between elasticity and density is not even approximately given by Boyle's law, but is apparently more complicated. It can, however, according to van der Waals *, be explained from the kinetic theory of gases, if the volume of the molecules and the attraction between them be taken into account.

Corresponding investigations in relation to the viscosity of gases have hitherto been carried out only so far as Kundt and one of us † have studied the deviations of Maxwell's law at very slight densities. For higher degrees of density the connexion between viscosity and density has not yet been investigated. For the solution of this problem (treated in the present paper for one substance, viz. carbonic acid) the corresponding values at constant temperatures of the coefficient of friction, the density, and, for many reasons, the pressure must be determined.

We employ as the measure of the pressure the inverse value of the volume of a mass of nitrogen at constant temperature of the apartment, the volume of that mass at the pressure of one atmosphere being put $=1$. To measure the pressure according to this definition a nitrogen-manometer was employed, which was always attached to the principal apparatus, and permitted pressures between 30 and 120 atmospheres to be evaluated.

The density of the substance heated above the critical temperature we determined by a volumetric measurement of the carbonic acid, which at each transition from a greater to a less density was liberated from our apparatus, the volume of which was known to us; the density of the mass in the apparatus after the conclusion of a series of experiments we calculated from the pressure, which then amounted to about 30 atmospheres, by Clausius's formula ‡, which at so small a pressure is sufficiently accordant with the observations. At the temperature 32°·6 our experiments comprise the interval of densities between 0·1 and 0·8.

For the determination of the friction-coefficient we employed the method of flow through capillary tubes. The capillary, placed vertical, ended below in a measuring-tube which dipped in mercury, above in a space A, which could from time to time be shut off from the rest of the space by a cock, and in which a diminution of pressure could then be

* *Dissertation*: Leyden, 1873. † Berlin *Monatsberichte*, 1875, p. 160.

‡ Wiedemann's *Annalen*, ix. p. 348.

produced by discharging carbonic acid. After the mercury had been thereby raised in the measuring-tube, the spaces A and B were again put into communication. From the time occupied by the mercury in the measuring-tube in descending from one mark to another, the coefficient of friction was calculated by means of the constants of the apparatus.

Three capillaries were employed, from 6 to 7 centim. in length, and of which the radii amounted to 0·005162, 0·003601, and 0·002847 centim. The validity of Poiseuille's law was controlled; but an equation cannot be deduced from the experiments.

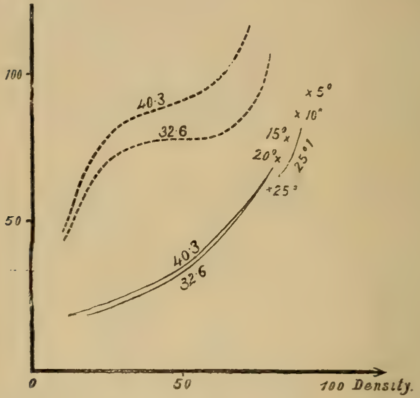
The results obtained are contained in the following Table, in which t denotes the temperature measured by the air-thermometer, s and μ the density and the friction-coefficient in the gramme-centimetre-second system, and p the pressure in the measure above-mentioned; λ is the air-content of the substance, in parts of a volume, as given by analysis. The density of the liquid carbonic acid is taken from Andreef's experiments*.

	$t=32°·6$. $\lambda=0·00074$.		$t=40°·3$. $\lambda=0·00085$.	
s.	p.	$\mu . 10^6$.	p.	$\mu . 10^6$.
0·800	107·3	677		
0·730	88·5	574	114·6	580
0·660	80·7	493	101·6	499
0·590	78·2	414	94·9	426
0·520	77·6	351	91·7	366
0·450	77·2	304	89·2	316
0·380	76·6	270	86·8	275
0·310	74·6	239	82·7	243
0·240	69·9	213	75·9	218
0·170	60·3	188	64·3	196
0·100	43·1	...	45·3	180

$t=25°·1$ $\lambda=0·00044$.		
p.	s.	$\mu . 10^6$
105	0·896	800
95	0·875	741
85	0·858	703
75	0·827	665
70	0·809	628

* *Annalen der Chemie und Pharmacie*, 1859, cx. p. 1.

Viscosity of liquid carbonic acid under the pressure of its saturated vapour. $\lambda = 0{\cdot}0018$.		
t.	s.	$\mu . 10^6$.
5°	0·922	925
10	0·895	852
15	0·864	784
20	0·827	712
25	0·783	625
29	......	539



The figure gives a graphic representation of the results—namely, the isotherms * of viscosity and tension, the latter in dotted lines, noted according to the Table.

Andreef's values of the pressure † exceed ours but little—at 32°·6, on an average, about one atmosphere. This may arise from the air-content of the carbonic acid being in Andreef's experiments somewhat less than in ours. The values of p calculated by Clausius's formula, however, are not inconsiderably higher than those observed by us; the differences increase with the density, and reach the value of 10–12 atmospheres. On account of the agreement of our results

* We thus name lines the abscissæ of which are proportional to the densities, and the ordinates to the friction-coefficients and pressures respectively.

† Pogg. *Ann.* Erg. Bd. v. p. 79.

with those of Andreef, found by a quite different method, it is not likely that the differences are due to errors of observation.

Respecting the viscosity, especially its connexion with the density, the results are as follows:—

I. *Above the critical temperature, Gaseous Carbonic Acid.*

1. To the maximum of compressibility $\left(\frac{1}{s\frac{dp}{ds}}\right)$, *i. e.* to the minimum of elasticity $\left(s\frac{dp}{ds}\right)$, given by observation corresponds no minimum of viscosity, which much rather increases in a constantly increasing ratio with increasing density.

$$\left(\frac{d\mu}{ds} \text{ and } \frac{d^2\mu}{ds^2} \text{ are always positive.}\right)$$

2. At the density 0·1, about 500 times the normal, the coefficient of friction exceeds the normal (0·000165 for 40°·3) by only about 9 per cent. of the latter.

3. At the temperatures 32°·6 and 40°·3 the substance shows, at equal density, slightly different values of μ, very different values of p. According to this the viscosity appears to be much more simply connected with the density than with the pressure.

4. The influence of the temperature upon the viscosity, at constant density, is so small that it cannot be inferred with perfect certainty from observations embracing a temperature-interval of only 8°. Since, however, the isotherm for 40°·3 runs entirely above that corresponding to 32°·6, the viscosity appears to increase slowly with the temperature when the pressure is constant.

II. *Liquid Carbonic Acid.*

5. Liquid carbonic acid showed far less viscosity than any other liquid hitherto examined. The friction-coefficient of water, for example, at 15° is 14·6 times that of liquid carbonic acid under the pressure of its saturated vapour. Even the appearance of liquid carbonic acid enclosed in a glass tube which is shaken excites the supposition that this substance possesses very little viscosity.

6. The viscosity of liquid carbonic acid at the temperature of 25°·1 increases with the density. By further extending this investigation, and especially to other liquids also, we purpose to ascertain the influence of temperature upon the viscosity of liquids at constant density—that is, the specific influence of temperature.

7. At densities in the vicinity of 0·8 the isotherm correspond-

ing to 25°·1 runs below both that corresponding to 32°·6 and those corresponding to 15° and 20°. From this follows that carbonic acid of such density, heated from 15° upward, must show a minimum of viscosity between 20° and 32°·6.

Poisson* has given a theory of liquid-friction which starts from the representation that, with respect to a system of simultaneous impacts, a liquid behaves, at the first moment after the expiration of them, like an isotropic solid body. Hence we can speak of the constants of instantaneous elasticity of a liquid. For the coefficient of friction in Poisson's theory we get the expression

$$\mu = \mathrm{K} \,.\, \mathrm{T},$$

where K is the coefficient of instantaneous rigidity†, and T a quantity of time which Maxwell has named the modulus of the relaxation-period. For an ideal gas, Maxwell finds‡ $\mathrm{K}=p$, and hence T, at constant temperature, proportional to the mean length of path.

In a first approximation let us assume that T still has this property when the volume of the molecules and the attraction between them is taken into account; then we get for μ a theoretical expression in which K alone remains unknown—namely

$$\mu = \mu_0 \,.\, \frac{\mathrm{K}}{\mathrm{P}} \,.\, \frac{s_0}{s}\left(1 - \frac{b \,.\, s}{\Delta}\right),$$

where for the temperature to which μ refers, μ_0 and s_0 denote the values of μ and s for the pressure P of one atmosphere. Δ is the normal density of carbonic acid, and b van der Waal's constant, namely four times the molecular volume, the volume of the substance at 0° and the pressure of one atmosphere being taken as the unit of volume. The equation holds so long as $s<2b$—that is, for carbonic acid, approximately as long as $s<0{\cdot}4$.

According to this equation, the occupation of space by the molecules produces a diminution of friction with increasing density, and consequently the opposite deviation from Maxwell's law to that produced by the attraction between the molecules. From the same equation, according to our experiments, for carbonic acid of density 0·38 at 32°·6, K comes to 7·2 kilograms upon the square millimetre—that is, about $\frac{1}{390}$ of its amount for glass, and somewhat more than for tallow§.

* *Journal de l'Ecole Polytechnique,* 1831, t. xiii. p. 139.

† In Kirchhoff's notation (*Vorlesungen,* p. 400).

‡ Phil. Mag. [4] xxxv. p. 211 (1868).

§ Pogg. *Ann.* cxxxvi. p. 295 (1869).

VI. *Notes on Thermometry.* *By* F. D. Brown, *B.Sc., Demonstrator of Chemistry at the University Museum, Oxford**.

[Plate II.]

SOME years ago, when I determined to try and find out something about the attractive forces which the atoms and molecules seem to possess, by studying the effects of heat upon chemical substances and upon mixtures of such substances, I was led to the conviction that, if the work which I proposed to do was to be of any permanent use, I should be obliged to take many and minute precautions regarding the measurement of temperatures—a measurement which, owing to the peculiarities of mercurial and other thermometers, is so liable to error. In order to learn how best to use my thermometers, and how to refer their readings to a satisfactory standard, I made a considerable number of experiments. At the time when these experiments were made I imagined that the subject of thermometry, although presenting many difficulties to my mind, had been thoroughly worked out by others, and therefore that a printed record of my observations would be generally deemed to be of little utility. The recent publication of a paper by Dr. E. J. Mills (Edin. Roy. Soc. Trans. 1880), of one by Professors T. E. Thorpe and Rücker (Phil. Mag. [5] xii. p. 1), and more especially of a report by M. Pernet (*Mém. et Travaux du Bur. inter. des poids et mes.* i. 1881, pp. 1–52), has led me to change my opinion, and to think that there still remain many points connected with thermometers about which not only I, but others also, would be glad to have more certain information. Acting upon this belief, I have put together in the following pages some of the results of my experiments.

The Mercurial Thermometer as a Standard.

I was soon convinced that any attempt to express temperatures in degrees of an ideal absolute thermometer, or even to refer them correctly to the readings of an air-thermometer, would involve a most extensive and wearisome investigation, which would postpone indefinitely the work I wished to do. To avoid this substitution of the means for the end, I decided to construct a mercurial thermometer and to use it as a standard, keeping it until such time as the progress of our knowledge should render its comparison with the air-thermometer a matter of less difficulty.

As a mercurial thermometer is very liable to be broken, I first wanted to know whether this instrument fulfilled the primary condition of a true standard, of being capable of

* Communicated by the Physical Society.

reproduction when lost or destroyed. With this end in view, I made two thermometers at different times, and wholly independently one of the other, and compared their readings. To those who may wish at any time to construct a mercurial thermometer without the elaborate appliances ordinarily employed, but in which absolute confidence may be placed, the following details may be of interest:—

A capillary tube of medium bore, about 800 millimetres long, free from all flaws, and having as uniform a section as possible, is provided with a millimetre-scale of 600 divisions. The etching of this scale is a matter of great consequence: it very frequently happens that the divisions on glass tubes are not of exactly equal length, but that, owing to some defect in the dividing-engine or some movement of the tube while undergoing the process of division, some of the divisions are so much longer or shorter than the rest as seriously to interfere with the subsequent process of calibration. Even when all the lines are equidistant, they are often so thick, and present so irregular an outline when viewed through a telescope, that it is impossible to fix upon any particular point as that represented by the dividing-line. The tubes I employed were selected and divided with special care by Mr. Casella, the lines being perfectly straight, less than 0·4 millim. in thickness, and in all cases equidistant.

As a glass tube, however carefully selected, is never of uniform bore, it is necessary to ascertain the relative capacities of the several divisions of the tube, or, in other words, to "calibrate" it. As is well known, this is easily done by placing a thread of mercury in successive positions along the tube and observing its length, the mean capacity of the divisions occupied by the thread being, of course, inversely proportional to that length. In this way, and by adopting the plan of correcting the position of the thread suggested by Dr. Mills in the paper above referred to, which plan he had been kind enough previously to communicate to me privately, a table is readily constructed showing the volume of the tube from the line marked 0 to any line marked n, and also the value of the succeeding division. The only difficulty connected with this process is the accurate measurement of the length of the thread of mercury in its several positions. It is true that this may easily be done with a dividing-engine or some similar instrument, such as a cathetometer provided with a micrometer eyepiece and placed horizontally. As, however, reliable instruments of this class are exceedingly costly, I designed a small piece of apparatus for the purpose, which has proved so convenient and useful that I venture to describe it here.

A mahogany board, B B (Pl. II. fig. 1), about 18 inches long and 4 inches wide, is provided with a groove, G G, of the shape shown in the section (fig. 1 *a*); a piece of gun-metal, about 5 inches long and $\frac{1}{4}$ inch thick, slides in this groove with some little difficulty—the friction, which is produced by the spring *f f*, being necessary to retain the plate rigidly in any given position. The plate, D, is provided with a slot, *e e*, and a millimetre-scale, S S, the dividing lines of which must, like those of the tube to be calibrated, be very fine and truly equidistant. The piece of gun-metal, E, which is provided with a vernier, carries the reading-microscope, M, and can be moved along S S by means of the rack and pinion *p*; the movement is rendered smooth and free from lateral displacement by the spring *c*, which causes the ends of E to remain always in contact with the straight edge of the slot. The thermometer-tube is fixed with suitable screws under the path of the microscope, so that the length of a thread of mercury can be easily measured by placing the microscope so that its cross wire coincides first with one end of the thread and then with the other, and noting on the scale the distance between the two positions.

The millimetres of the brass scale and those of the tube, if marked off by different makers, will often differ a little in length; hence it is generally more satisfactory to obtain from the glass scale the number of whole divisions occupied by the thread, and to measure the terminal fractions only by the microscope.

Since the line on the outside of the tube is nearer the eye than the thread of mercury inside the tube, it is clear that when the microscope is adjusted to view the end of the thread, and is then moved along until the cross wire coincides with the nearest line, this last will be out of focus, and either the whole microscope must be raised up or the distance between the object-glass and eyepiece altered. Now, unless the instrument be constructed with great solidity, and much care be taken to fit accurately all the moving parts, this adjustment will probably alter the position of the optical axis, and so render the measurements inaccurate. To avoid this difficulty, I added a half-lens, L, fitted in the ordinary way on a brass tube sliding on the end of the microscope. This lens of course brings the focus of half the field nearer the object-glass; so that, by properly adjusting it, the divisions are seen through the half-lens at the same time that the mercury is observed through the unprotected part of the object-glass. In this way all disturbance of the microscope is avoided throughout the calibration, which is thus carried out with much greater comfort and accuracy.

Two tubes were calibrated with this apparatus, and tables of their volumes from the first division compiled; they were then furnished with bulbs, filled with mercury, and sealed up in such a manner that they formed thermometers capable of indicating temperatures between 0° and 150° C. The fixed points of the two thermometers having been determined with the precautions indicated below, tables showing the temperatures corresponding to the readings of the scale were made in the usual manner; the two instruments were then compared together, either in a large tank of water which was kept well stirred, or in the steam-apparatus which I described to the Physical Society at the time when these experiments were made. Before a series of readings were taken, both thermometers were heated for at least half an hour in steam, while their zero-points were observed after the series was completed. The numbers given in the following table show that the two thermometers gave practically identical readings. It would seem, therefore, that the mercurial thermometer, when carefully made and systematically heated, does really possess that valuable property of a standard, of being capable of exact reproduction.

Reading of AS, corrected for index-error.	Reading of BS, corrected for index-error.	Corresponding value of AS, in degrees.	Corresponding value of BS, in degrees.	Difference.
58·55	70·64	14·30	14·29	−·01
134·33	150·11	33·69	33·71	+·02
179·69	197·20	45·29	45·30	+·01
321·97	345·96	81·88	81·88	·00
23·42	33·74	5·28	5·28	·00
26·33	36·83	6·03	6·04	+·01
30·07	40·63	6·99	6·97	−·02
33·76	44·57	7·94	7·93	−·01
43·32	54·70	10·40	10·40	·00
47·98	59·60	11·59	11·59	·00
69·42	82·13	17·09	17·09	·00
91·61	105·52	22·78	22·79	+·01

Determination of the Zero-point.

In most books on physics it is stated that, in order to obtain the zero-point of a thermometer, the instrument should be placed in a vessel filled with broken ice and provided with holes at the bottom, through which the water formed by the melting of the ice may escape. In order to learn whether this method is the best possible, the following experiments were made:—A number of tin pots, about 7 inches high and 4 inches in diameter, were obtained, and holes made in the bottoms of two or three of them. A large block of ice was

broken up into small fragments, which were well mixed up, so as to render the whole perfectly uniform in character. One of the tin pots, which we will call A, was filled with some of this ice, which had been washed in a funnel with ordinary water; A was then filled up with water, so as to form a mixture in which the ice largely predominated. A second tin, B, was filled with some more of the ice, which had been washed with ordinary water in the same way; B, however, had holes at the bottom, and the water formed by the fusion of the ice thus drained away. A third tin, C, contained some of the same ice, which had been washed in a funnel with distilled water, and then mixed with distilled water in the same way as in A the ice was mixed with ordinary water. In a fourth tin, D, which was provided with holes, some ice was placed which had been washed with distilled water. Finally a quantity of distilled water was artificially frozen, the ice broken up into small pieces, washed, and mixed with distilled water in a fifth tin, E. A thermometer with a long narrow bulb, and with a stem divided into millimetres, was carefully inserted into each tin in succession, and readings taken with a cathetometer. About 17 millim. of the scale were equivalent to one degree Centigrade. In A the readings soon became constant at 1°·00; in B the readings varied considerably for about half an hour, but finally became constant at 1°·12; in C the thermometer became rapidly constant at 1°·16; in D the readings became constant after a short time at 1°·06; in E the readings did not vary after the first four or five minutes, remaining at 0°·64.

At the end of these observations, which occupied nearly two hours, the thermometer was replaced in A, where the mercury rapidly assumed the same position as before, viz. 1°·00. Seeing that, with the exception of E, the greatest difference in the readings does not amount to 0°·01 C., we may fairly draw the following conclusions:—First, that a constant temperature is more rapidly and certainly obtained with a mixture of ice and water than with ice alone; secondly, that the temperature thus obtained is really that of melting ice; thirdly, that it is preferable to wash and mix the ice with distilled water, ordinary water tending to lower the temperature, though to an insignificant extent.

With the view of seeing whether different varieties of ice gave the same results, two specimens of block ice and one of the rough thin ice collected in winter near London were obtained, while two cylinders of distilled-water ice were artificially produced. These were all broken up separately into small pieces, washed with distilled water, and then mixed with

the same in five tins, A, B, C, D, E. The thermometer placed in these tins marked 1°·30, 1°·34, 1°·26, 1°·30, and 1°·27 respectively (these numbers are not comparable with the former, as the experiments were made a month or so later, when the zero of the thermometer had altered its position). These experiments showed that distilled-water ice gave the same results as ordinary ice, and that the melting-point of different specimens of ice, when mixed with distilled water, was the same within 0°·005 C. The exceptionally low reading obtained with the tin E in the first series of experiments was probably due to the fact that the ice, having been made by means of a freezing-mixture, was not at its maximum temperature.

In subsequent determinations of the zero of thermometers I have always used ordinary block ice, washed and mixed with sufficient distilled water just to fill up the spaces between the pieces, and have not allowed the water to drain away. These results are in accord with those obtained by M. Pernet.

Zero-movements, and Substitution of the Determination of the Steam-point for that of the Zero-point.

In considering the well-worn question of the zero-movements of thermometers, it is important to distinguish between its practical and theoretical aspects. To make a study of zero-movements from an abstract point of view, to find out equations expressing these movements under different circumstances and with different thermometers, to learn that when a certain thermometer has been subjected to a certain series of temperatures at certain intervals of time its indications on next changing its temperature will be affected with a certain index-error, may possibly be of some utility, but it does not aid us much in the endeavour to free the readings of thermometers from the errors with which they are surrounded. When once we have acquired the information that a thermometer subjected only to those changes of temperature which are due to the weather exhibits a gradual rise of zero, that the rise thus taking place in a given time diminishes as the age of the thermometer increases, but differs for different thermometers, when we also know that a thermometer subjected to a high temperature after a considerable period of rest exhibits a decrease in its zero-reading, dependent on the thermometer itself and also on its previous history,—we know all, or nearly all, that we can put to practical use.

Thus, for example, the thermometer attached to my standard barometer was verified at Kew Observatory when it was first supplied to me, some four or five years ago. Since then I have from time to time observed its reading in melting ice,

and have modified accordingly the correction to be applied to it. Now, no observations of other thermometers—no curves or equations representing their zero-movements—could be of any assistance to me in this matter. I knew that the zero would probably rise, and that the amount of the rise would not be the same in my case as in that of others, and that, therefore, I must obtain the index-error experimentally. I also knew that if I boiled the thermometer I should cause irregular changes in the position of the zero; and as there was no necessity for the operation, I avoided boiling it. But if by mischance it had fallen in boiling water, no equations representing the zero-movements of other thermometers would have told me exactly what had happened to mine; I should simply have been obliged to observe its index-error more frequently than before the accident happened.

The question which seems to me to be of the greatest importance with regard to zero-movements is, how we can best reduce the trouble which they cause us. In the case of all meteorological and clinical thermometers, where the changes of temperature are small, as in the above case, it is evident that all we can or need do is to protect the instrument from unnecessary changes of temperature. When, on the contrary, our observations extend over wide ranges of temperature, the difficulties increase considerably. Suppose, for example, that I want to use a thermometer to indicate accurately a series of temperatures between 70° and 90°. It is obvious that if I observe the index-error beforehand, and apply the correction thus obtained to my readings, I shall not be doing right; for the very heating of the thermometer to 70°–90° will have altered the index-error. But if, on the other hand, I first heat the thermometer to 100°, then ascertain its index-error, then make my experiments with it, and finally observe its reading in ice a second time, I shall be tolerably certain, if the index-error is the same at the end as at the beginning of the experiment, that no variation has occurred during the observations.

In most laboratories, however, the frequent determination of the zero-point of a thermometer involves a considerable expenditure of labour: ice has to be purchased, broken up into small pieces, washed, and placed in a suitable vessel. All this requires no little time, and has, moreover, to be repeated at every determination, since the broken ice melts away in the interval. On the other hand, the apparatus for the observation of the steam-point is always in readiness; if, therefore, no greater error arises when the index-error is determined before and after the experiments by means of the steam-point, a great saving of time will be effected, without any corresponding loss of accuracy.

When the temperatures to which the thermometer is to be exposed are greater than 100°, the instrument should be heated for some time to the highest probable temperature before the steam-point is observed for the first time. In this way the lowering of the zero which takes place when a thermometer is heated from 100° to some higher temperature, to which it has not been exposed for some time previously, is effected first of all, and does not take place during the experiments, as it otherwise would.

The only objection which can be raised to this method is that, when some at least of the temperatures to be measured are below 100°, it is possible that the steam-point, which is lowered by the first heating in steam, rises again during the experiments (that is, when the thermometer is at a lower temperature), and then, by the second heating in steam, is again brought to the same position as at first. In this way the observations in steam, although concordant, would not give the true index-correction to be applied to the readings. That the error which thus arises is of no importance is, I think, rendered probable by the following considerations:— The gradual rise of the zero of a thermometer receives its most natural explanation when it is supposed that the glass bulb, after having been heated and somewhat quickly cooled, is in a state of strain which causes it to have a larger capacity than it would have if no such strain existed. As time goes on, and more especially as the thermometer is subjected to small fluctuations of temperature, the particles of the glass gradually yield to the forces which are acting upon them, and take up new and more suitable positions. These molecular movements result in a gradual diminution of the capacity of the bulb, and consequently in a rise of the zero. Now it is evident that, if a certain state of strain is set up when a thermometer is cooled from 100° to 0°, when it is cooled from 100° to some intermediate temperature t the strain set up will be less considerable; there will therefore be a greater tendency for the zero to rise when the thermometer is placed in melting ice than when it is subjected to the temperature t. Consequently, if it be found that, when a thermometer after being heated in steam is placed in ice, no change of the zero takes place for three or four hours afterwards, we may legitimately conclude that, if the thermometer were maintained for the same time at the temperature t, no movement of the zero would occur. I have frequently kept recently-heated thermometers in melting ice for several hours, renewing the ice when necessary; and I have always observed, with all of my instruments, that no change took place for the first three hours, and that

during the next two or three hours the rise was extremely small. It follows, therefore, that if in any series of observations lasting more than three hours the thermometer be heated in steam at the end of every third hour, there will be no uncertainty as to the position of the zero; that if the experiments be carried on continuously for six hours, a slight rise of the zero may occur during the last part of the time, but that this rise will not amount to more than one or two hundredths of a degree.

Correction for the Exposed Portion of the Thread.

When a thermometer is only partially immersed in the medium of which the temperature is to be observed, the readings become subject to an error which arises from the fact that a part of the thread of mercury, together with the corresponding portion of the stem, are at a temperature different from that of the bulb and immersed portion of the stem. The correction, C, usually applied in this case is given by the formula

$$C=m(T-t)N, \quad \ldots \ldots \quad (1)$$

where T = the reading of the thermometer,
t = the temperature of the exposed portion,
N = the number of exposed divisions of the stem which are filled with mercury,
m = the apparent expansion of mercury in glass.

This formula is founded on the assumption that the error in the reading has no other cause than the comparatively unexpanded condition of a portion of the thread and stem.

The apparent expansion of mercury in glass, as obtained from Regnault's experiments, is about ·0001545; but it differs, of course, for different specimens of glass. When this number is employed in the above formula, the values of C obtained are generally believed to be too large; indeed a little reflection will convince us that this must be the case whenever the temperature of the exposed portion is merely measured by placing another thermometer with its bulb halfway up it. This second thermometer evidently measures the temperature of the ascending stream of warm air around the stem; if the stem of the chief thermometer were subjected to the heating influence of this stream, and to no other, its temperature would be rightly given by the subsidiary thermometer; but the thermal conduction along the thread of mercury and along the glass stem must necessarily raise the lower part of the

exposed stem to a temperature higher than that indicated by the subsidiary thermometer. The value of $(T-t)$ therefore is too great, and consequently also that of C.

In order to meet this difficulty, Dr. Mills, instead of endeavouring to give to $(T-t)$ its proper value, has made a large number of experiments with different thermometers with a view to assign a more satisfactory value to m, and has thus been led to draw the following conclusions:—The value ·0001545 of the coefficient m is invariably too great. This coefficient varies with the thermometer employed, and also with the number of divisions of the thread exposed; so that, instead of assigning one definite value to m for each thermometer, we must give it a value

$$m=\alpha+\beta N,$$

where α and β must be determined for each thermometer.

Professors Thorpe and Rücker, on the other hand, while admitting that the value $m=\cdot 0001545$ may be generally too large, maintain that it is sufficient to replace it by some other single number, and that the employment of the varying coefficients $\alpha+\beta N$ is unnecessary; they support this opinion by showing that in Dr. Mills's own experiments the alterations in the value of C, caused by the introduction of the term βN, do not amount to more than one or two hundredths of a degree, and are therefore insignificant. Dr. Mills, replying to this, states that the change in the correction C brought about by the term βN often amounts to so many hundredths of a degree that it cannot be neglected.

Now it is clear that by merely placing a second thermometer halfway up the exposed thread, only the roughest idea is obtained of the real temperature of the thread. Suppose, for example, that $T=100°$, and that t is taken at 15°, being subject to an error of 5°: the value of $(T-t)$, which is 85, will be subject to an error of 5°, or about 6 per cent. What, therefore, can be the use of attempting to determine the coefficient β, of which the value would appear ordinarily to be about 0·0000002, when so great a source of error is left unprovided for?

In all experiments in which I have had occasion to use mercurial thermometers, I have endeavoured to avoid any correction for the exposed thread, by making the apparatus and thermometers employed of such relative dimensions that the whole thread and bulb, except the topmost division, are at the same temperature. When this is impossible, and when the experiments require such extreme accuracy, it seems to me that the first thing to be done is to surround the exposed portion of

the thread with a current of running water, and so, while preserving it from the uncertain effects of conduction, radiation, &c., to render possible the observation of its exact temperature. The value of $(T-t)$ being thus correctly measured, that of m is found to be constant for all values of N, and to differ but little from 0·001545. It varies, however, with different thermometers.

The following experiments show most distinctly the truth of this statement:—

One of the standard thermometers mentioned in the first section of this communication was partially surrounded by a glass tube, *a b* (fig. 2), about an inch in diameter; this tube was closed at the bottom with a piece of good cork, about 8 millim. thick, through which the stem of the thermometer passed. The upper end of the tube *a b* was fitted with a cork, in which were four holes—one for the stem of the chief thermometer, a second for a thermometer to indicate the temperature of the water contained in the tube, while through the two others passed the tubes by means of which the current of water was maintained. The thermometer thus furnished was fixed vertically in the ordinary apparatus, A, for determining the 100°-point of thermometers. The open end of A was closed with a thin disk of brass, with a small central hole, through which the thermometer passed. One degree was equal to about four divisions of the millimetre-scale of the thermometer, the readings of which were observed with a cathetometer, and the fractions of a division measured with that instrument. It was found that the readings of the thermometer under these conditions were correct to ·02 of a millimetre, or ·005 of a degree. The numbers given below are the means of three readings, which, however, were nearly always identical. The thermometer in the water was graduated to fifths of a degree, and had been compared with the standard.

The chief thermometer was first heated in the steam for an hour, with two or three inches of the thread above the cork; it was then pushed down until the quicksilver was only just visible above the cork, and the reading noted; it was then pulled up again, and readings taken in various positions, as given in the following table; finally the thermometer was again pushed down as far as possible, and the reading taken, when it was found to be the same as before, showing that no change in the 100°-point had supervened during the experiment. Of several series of observations made in this manner, the one contained in the following table will suffice, since they all led to precisely the same result.

Reading of Standard when wholly immersed $=393 \cdot 42$.
{ Barometric pressure, corrected and reduced, $=760 \cdot 1$.
Corresponding temperature of steam $=100^{\circ} \cdot 00$. }

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard, T.	Value of C $=393 \cdot 42-\mathrm{T}$.	Value of m, $=\frac{\mathrm{C}}{(\mathrm{T}-t)\mathrm{N}}$.
317	$13^{\circ} \cdot 0$	389·01	4·41	·0001599
277·5	12·3	389·54	3·88	·0001594
221	12·0	390·30	3·12	·0001604
173	12·1	390·94	2·48	·0001631
130	12·1	391·58	1·84	·0001610
79	12·1	392·30	1·12	·0001612

An inspection of the above table is sufficient to convince us that the value of m is constant, and equal to the apparent expansion of mercury in the glass of which the thermometer was made; the numbers would probably have agreed even more closely, were it not that it is impossible to arrange the apparatus so that the cold portion of the thermometer-stem follows directly upon the hot portion. There must always be an interval occupied by the cork, the temperature of which is uncertain. It should be remarked that there is no indication whatever of the value of m increasing when that of N increases.

Precisely the same results were obtained with the second standard thermometer, as is shown by the following table:—

Reading of Standard BS, when wholly immersed, $=419 \cdot 21$.
{ Barometric pressure, corrected and reduced, $=760 \cdot 5$.
Corresponding temperature of steam $=100^{\circ} \cdot 02$. }

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard BS, $=\mathrm{T}$.	Value of C, $=419 \cdot 21-\mathrm{T}$.	Value of m, $=\frac{\mathrm{C}}{(\mathrm{T}-t)\mathrm{N}}$.
302	$12^{\circ} \cdot 0$	415·13	4·08	·0001535
237	11·9	415·96	3·25	·0001556
174	12·0	416·84	2·37	·0001548
127	12·0	417·47	1·74	·0001557

Here, again, the value of m varies only within the limits of

the error of observation, and shows no tendency to increase when N increases. It may be noted that with both the above thermometers the mean value of m is *greater* than ·0001545, the value usually assigned to it, but that it differs from that number by so little that the error committed by substituting the one for the other in the calculation of the correction C will rarely amount to more than 0°·02 C.

The above experiments were made at 100°, because this is the only temperature which can be maintained absolutely constant for an hour without the use of a quantity of complicated apparatus; and it is evident that the slightest variation in the temperature would entirely spoil the series of observations. At higher temperatures the sources of error which beset the readings of thermometers increase so rapidly that the exact value of the coefficient m becomes of less and less importance as the temperature rises, notwithstanding the fact that the correction C increases in amount. Since there is no reason whatever to suppose that any different results would be obtained at such higher temperatures, I thought it unnecessary to make any further experiments, more especially as those given above yielded precisely those numbers which the ordinary laws of expansion predicted.

There is another point connected with thermometry, to which I devoted attention some years ago. It has been suggested that when a thermometer is placed in a vapour at maximum tension, as in the ordinary chemical process of distillation, it does not truly indicate the temperature of the vapour. This suggestion owes its origin to the fact that drops are seen to accumulate and drop off the end of the thermometer. It has been supposed that this condensation of the vapour on a surface which should be as hot itself, is due to the molecular attraction of the glass for the vapour. If this be the case, the heat evolved by the vapour during liquefaction on the thermometer-bulb would raise the temperature of the latter. The thermometer would thus indicate a higher temperature than that of the mass of the vapour. The experiments which I made upon this subject, like those instituted by others, were inconclusive. I possess, however, an apparatus which seems to me eminently suited to answer the question satisfactorily. It is at present being employed for other purposes; but I trust that, when it is at liberty, I shall be able to put it to this not unimportant use.

VII. *Notices respecting New Books.*

Geological and Natural-History Survey of Canada. By Alfred R. C. Selwyn, *LL.D., F.R.S., Director. Report of Progress for* 1879–80. Montreal: Dawson Brothers, 1881.

THE Geological Survey of Canada, under the direction of Dr. Selwyn, appears to make steady progress. This volume refers mainly to the work of the Geological Corps during the season 1879–80, which embraced further explorations in parts of the North-west Territory (the Souris-River Coalfield), Hudson's-Bay basin, Quebec, New Brunswick, Nova Scotia, and the Magdalene Islands, as well as the results of the survey in 1879 by Dr. G. M. Dawson of a portion of the northern part of British Columbia, and of the Peace-River country, which will be found to embody the best and most reliable information on this vast and interesting region.

The other Reports comprise many valuable details bearing on the physical features, geological structure, climatal conditions, soils, and economical minerals of the different provinces explored, of which that on Hudson's Bay by Dr. Bell (in continuation of the survey of the two previous years) is very interesting; for perhaps comparatively few people have any adequate conception of the extent of this great Canadian sea, which is the central basin of the drainage of North America. Geologically this basin, excluding the Winnipeg division (the rocks of which range from the Laurentian to the Tertiary), lies within the great Laurentian area of the Dominion; resting upon these, the Cambro-Silurian rocks form an irregular border along the south-western side of the bay; while to the south and west of James' Bay these latter are overlain by Devonian rocks, which occupy a considerable area. The chains of islands which fringe the east coast to the northward of Cape Jones, and also the mainland near Richmond Gulf, are composed of bedded volcanic and unaltered sedimentary rocks, which may be of Lower-Cambrian age; on the western side the quartzites and other rocks, rather largely developed, probably belong to the Cambrian system. The Reporter considers that few of the varied and numerous resources of Hudson's Bay are at all developed, that the fur-trade and oil are the principal; but that the most important of the undeveloped resources are the soil, timber, and minerals, and that the latter may become in future the greatest of the resources of the shores of Hudson's Bay. The concluding part contains a very suggestive paper (with a map) on the northern limits of the principal Forest-trees of Canada. In the Report of British Columbia, Dr. Dawson also gives an account of the distribution of the more important forest-trees of that province.

The geological structure of Northern New Brunswick and Eastern Nova Scotia is fully described in the respective Reports. The Lignite Tertiary formation in the Souris valley (North-west Territory) is reported on by Dr. Selwyn, with appendices on the nature of the strata and their plant-remains by Dr. G. M. and

Principal Dawson. In the western prairie-region the true Carboniferous system is not the coal-bearing formation; and although lignites and coals are known at several different stages of the Cretaceous rocks, it is in the representative of the Fort-Union rocks of the U.-S. geologists that the most extensive and numerous beds of lignite of the Souris-River region occur, and constitute the nearest available supply for the province of Manitoba. "The flora of the Great Lignite Tertiary series of the North-west, though undoubtedly similar to the Miocene of Europe, really characterizes the beds which in the West constitute the transition from the Cretaceous to the Tertiary, and which form one great continuous series, probably on the horizon of the Eocene of Europe, though with local differences which are liable to be mistaken for differences of age" (p. 55 A.)

With regard to the origin of certain granites, Dr. Selwyn (p. 5), in describing the granites along Maine and New-Hampshire boundaries, says that there is absolutely no proof that these later granites are "intrusive," as so designated by Sir W. Logan, but that, like those in Australia and Nova Scotia, all the phenomena connected with them may be more readily explained and understood if we regard them as completely metamorphosed portions of the strata which now surround them; whereas in regions where the granite is older than the adjacent strata similar contact-lines may be seen, but without any change in the mineralogical character of the latter such as occurs when the crystalline rock is the youngest (p. 6).

Although the subjects of these Reports are locally important, they will doubtless be of interest to those who may wish to become acquainted with the physical conditions, geological structure, and economical resources of the Dominion of Canada.

Geological Sketches at Home and Abroad. By A. GEIKIE, *LL.D., F.R.S., Director-General of the Geological Surveys of the United Kingdom.* With Illustrations. London: Macmillan and Co. 1882.

THIS work consists of a series of essays previously published in various journals during the last twenty years. Most of them relate to certain districts in this country, Europe, and America, where the striking geological phenomena here recorded were observed by the author. Among the more important subjects noticed in these papers are those having more or less reference to Denudation, Glacial action, Volcanic phenomena, and Rock-weathering. An Erosionist of the advanced school, but by no means inclined to do battle under the extreme "quietest" banners of some of its champions, Dr. Geikie has been led in his wanderings to look at scenery with peculiar interest; and thus, in the essay on the Old Glaciers of Norway and Scotland, it is shown that the Norwegian and Scottish Highlands seem to be but parts of one long tableland of erosion composed of older and chiefly metamorphic rocks, while the fjords and valleys of the one country and the lochs and glens of the other owe their excavation to the great process of denudation

which has brought the land to its present form. The various forms of rock-weathering are discussed as derived from the study of tombstones (Essay viii.); and the effects of modern atmospheric action, conjoined with the bedding and jointing of rocks, are shown in the isolated pillar of Old Red Sandstone, 600 feet high, standing out from the mainland of which it once formed a part, and known as the "Old Man of Hoy" (No. I.).

The power of rain- and river-action is fully shown in the deep gorges excavated through the basalt and other rocks of the Auvergne and Haute-Loire of Central France (No. V.); but great as has been the efficiency of superficial erosion on the development of the terrestrial surface of Europe, the author considers that the fundamental laws of denudation can nowhere be better learnt than in the western region of the Rocky Mountains, the evolution of the mountain-forms of the Uintah range, the high plateaux of Utah, and the great basin of the Colorado, where "the proofs of enormous superficial waste rise to such a gigantic scale as wholly to baffle every observer who has yet attempted to describe them" (p. 229).

In the essays on the Volcanoes of Central France (No. V.), the Yellowstone Geysers (No. X.), and the Lava-fields of North-west Europe (No. XI.), are many interesting and suggestive remarks on volcanic phenomena. The study of the former seemed to throw light on the character and aspect of the Carboniferous volcanos of Central Scotland (p. 102). Again, there were features of former volcanic action on which the phenomena of modern volcanos appeared to afford but little light; "in particular, the vast number of fissures which in Britain had been filled with basalt and now formed the well-known and abundant 'dykes,' appeared hardly to connect themselves with any known phase of volcanism" (p. 276). This has been accounted for by the emission of vast floods of lava, "massive eruptions," without the formation of cones or craters—a view advocated by Richthofen more than twelve years ago,—and that our modern volcanos, Vesuvius and Etna, present us by no means with the grandest type of volcanic action. Dr. Geikie, after his visit to the lava-fields of the Pacific slope, was enabled to realize the conditions of volcanism described by Richthofen, and thus assist in solving a difficulty he had long felt in accounting for the extent of the dykes and other protrusions of basalt, "which can be traced over an area of probably not less than 100,000 square miles in Britain; for they occur from Yorkshire to Orkney, and from Donegal to the mouth of the Tay" (p. 276), which was only part of the far more extensive region that included the Faröe Islands and Iceland.

These stupendous outpourings of lava in the west of Scotland, like those on the plains of Idaho, are considered to be due to the fissure or "massive" type of eruption; and that the basaltic plateaux of Abyssinia and the "Deccan traps" of India probably mark the sites of some of the great fissure-eruptions which have produced the lava-fields of the Old World (p. 285).

Although Dr. Geikie advocates the fissure-eruption theory in explanation of certain volcanic phenomena, it must be remembered that Mr. Scrope, in his energetic review of the Natural History of Volcanic Rocks, strongly opposed Richthofen's twofold division of volcanic rocks into "massive eruptions" and the products of "volcanos proper," and remarked:—"It is utterly impossible to find in the writings of its advocates or in nature any intelligible distinction between volcanic rocks that have issued from fissures, so as to form 'massive' or 'elongated' or 'dome-shaped' mountains, and rocks produced by eruptions from 'volcanos proper'" (Geol. Mag. vol. vi. p. 512).

The last three chapters include lectures on the Scottish school of geology, Geographical evolution, and on the Geological influences which have affected the course of British history.

These well-written essays, now collected and revised, fully convey in a clear and pleasant manner the vivid impressions made on the author during his geologic wanderings, and are replete with scientific facts, occasionally interspersed with notes and illustrations of the striking features of the scenery or of historic and legendary interest.

VIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from Vol. xiii. p. 375.]

April 26, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On Fossil Chilostomatous Bryozoa from Mount Gambier, South Australia." By Arthur W. Waters, Esq., F.L.S., F.G.S.

2. "*Thamniscus*: Permian, Carboniferous, and Silurian." By George W. Shrubsole, Esq, F.G.S.

3. "On the Occurrence of a New Species of *Phyllopora* in the Permian Limestones." By George W. Shrubsole, Esq., F.G.S.

4. "On the Relations of the Eocene and Oligocene Strata in the Hampshire Basin." By Prof. John W. Judd, F.R.S., Sec.G.S.

The section at Whitecliff Bay, in the Isle of Wight, affords us the means of determining the true order of succession of nearly 2000 feet of Tertiary strata, and is therefore employed as a standard to which to refer the strata seen in sections where the order of succession is not so clear. The author supported the views of Prof. Prestwich as to the limits of the Bracklesham series, as opposed to the opinions expressed on the subject by the Rev. O. Fisher. He pointed out the confusion which has arisen from the correlation of certain strata in the Hampshire basin with the barren Lower and Upper Bagshots of the London area, in which fossils are so rare as to render their geological age somewhat doubtful. To the Lower Bagshot some authors have referred 660 feet of the strata seen at

Alum Bay; while other authors have restricted that name to about 73 feet of the same section. The age of the Upper Bagshot of the London basin is admitted by all authors to be very doubtful. The only way to avoid the confusion unavoidable from using the same names for strata the correlation of which was so hypothetical, was to employ local names for both sets of beds. He proposed to refer to the freshwater sands below the Bracklesham and Bournemouth strata, containing a distinctive flora, as "the Studland beds," and the sands above the Barton clay by the old name of "the Headon-Hill Sands."

Above these sands are a series of clays only about 40 feet thick at Whitecliff Bay, but much thicker at Headon Hill and Hordwell Cliff. These sands and clays form the Headon group; they consist of freshwater strata with bands of limestone and lignite, but including numerous inconstant intercalations of layers containing marine shells, for the most part much dwarfed. The age of the Headon group, as shown by the fossils which it contains, is that of "the zone of *Cerithium concavum*" of continental authors.

The brackish-water Headon group is succeeded at Whitecliff Bay by nearly 100 feet of purely marine strata. These marine beds, which had been shown to rest on an eroded surface of the Headon beds, contain the remarkable fauna which had been recognized by many British and foreign geologists as that of the Lower Oligocene. Similar strata with the same fossils are found in the New Forest, at Lyndhurst, Brockenhurst, Roydon, and other points, and there also attain a considerable thickness. It was pointed out that this marine series is quite distinct from the Headon, or zone of *Cerithium concavum*, with which it had been confounded.

The author had been very severely criticised for the views which he had put forward in a former paper as to the manner in which the Brockenhurst series is represented in the section at the west end of the Isle of Wight. There was much difficulty in these variable estuarine beds in correlating the beds seen in Colwell Bay with those exposed in the cliffs of Headon Hill. With several previous authors on the subject, he maintained that the great series of sandstones and limestones forming Warden Point and How Ledge are continuous with those exposed in the face of Headon Hill, and, consequently, that the marine beds of Colwell Bay overlying these limestone series are younger than the brackish-water bands interstratified with the Heddon beds of Headon Hill. His critics, however, insisted that these two beds agreed with one another in such a manner that they must be regarded as parts of the same bed, separated by denudation. In opposition to this view it was pointed out that the Colwell-Bay bed is of the most inconstant character, and long before reaching Headon Hill is seen to be on the point of thinning out and disappearing altogether.

In conclusion, the author pointed out that his own interpretation of the succession and correlation of the strata in the Hampshire basin brings them into complete harmony with that which is maintained by the great majority of continental geologists, while that of his critics appeared hopelessly irreconcilable with their views.

IX. *Intelligence and Miscellaneous Articles.*

SCIENCE AND METAPHYSIC.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you allow me space to say a few words upon some points which are raised and discussed in your review of Prof. Max Müller's translation of Kant's 'Critique of Pure Reason.' The first question of importance, namely that of the meaning of metaphysic and its relation to science, is an extremely interesting one, and, as it seems to me, well worthy of discussion from a scientific standpoint. One side of the question is put forward in your review; and it is the other side which I wish briefly to state here, with the object of eliciting discussion.

Metaphysic, as now understood, and Science are not words which are opposed as regards meaning. The subject-matter of science and of metaphysic are distinct: they run in different, yet closely connected channels. And perhaps here, at the outset, I may be permitted to remark that, in any sense in which the word metaphysic is or has been held, it is inadmissible to instance Auguste Comte as a metaphysician, even if his own reiterated statements upon the question did not forbid. Any one who has read the *Philosophie Positive* will know how Comte never lets an opportunity pass without ridiculing both metaphysic and metaphysician, in the old sense of the words. He regarded the metaphysical method of looking at things as a stage through which the human mind has to pass before it reaches the final stage, which he called the positive or scientific. In fact, one of the main objects of his great work was to free science from metaphysic as he understood it. When Comte censured as useless the study of the fixed stars with the object of discovering their chemical composition, he was arguing upon grounds that were of the most commonplace and superficial kind, and could not by any means be shown to be even remotely connected with metaphysic. I am unable to agree, except in a very limited sense, with Prof. Max Müller in thinking that there is any connexion between the philosophical systems of Kant and Comte. Metaphysic, until quite recent times, has been mainly an attempt to discover the supposed hidden causes of phenomena; a mode of inquiry which had its systematic origin in Aristotle, and was extended and formulated by the Schoolmen. In science the same tendency was manifest. Abstract entities were assumed as originating causes; and it is only recently that science has given up the search for these causes as futile, and sought to show how phenomena take place instead of why they take place. Here was a change of method in science; and what I wish to emphasize is that a similar change has occurred in metaphysic. Metaphysic has adopted the new method. While formerly the metaphysician endeavoured to discover what the facts of nature were in their hidden essence, he now seeks to find out what these facts are known as or appear

to be; in fact his whole purpose is to arrive at a complete analysis of the contents of the mind. The constant question in metaphysic is, What do we *mean* by such words as Time, Space, Cause, Necessity, Power, and other facts which are taken as ultimates by Science? It does not aim at discovering the cause of these facts, but simply at studying their relations. The introduction of this method is mainly due to Kant; and it is on this account that his claim to so high a place in the history of philosophy rests, although his method still retained a large admixture of the old conception. This new method of philosophizing without the assumption of entities, which we may call the New Metaphysic, will be understood by reference to such works as Mr. Shadworth Hodgson's 'Philosophy of Reflection,' or M. Renouvier's *Essais de Critique Générale*. What may be the merits of the systems propounded in these books, taken as a whole, I do not here discuss; but of the former of them, at least, it may be said that there is no conclusion touching scientific questions which Science, as such, would hesitate to indorse.

Yours truly,

WYNDHAM R. DUNSTAN.

ON THE DEPRESSION OF THE ZERO-POINT IN MERCURIAL THERMOMETERS. BY J.-M. CRAFTS.

It is well known that a thermometer which has remained for a long time at the ordinary temperature presents when it is heated a depression of its zero-point. The amount of the depression depends on the temperature to which the thermometer is raised; and M. Pernet has demonstrated that the depressions are proportional to the squares of the temperatures when it is heated from 0° to about 100°. For higher temperatures Mr. Mills has found a very different course. He does not indicate all the details which would be necessary to enable one to usefully criticise his data; and I will confine myself to saying that the numbers he gives resemble those obtained with thermometers which have not undergone a suitable preparation, and that the method of experimenting which I desire to present to the appreciation of the Academy leads to very different results.

Before measuring the depressions produced by a reheating, it is necessary to make sure of two things:—(1) that the starting-point is the zero raised to the maximum; (2) that the phenomenon one wishes to observe is not masked by the permanent elevation of the zero, which may take place at the same time as the depression. I have ascertained that this last effect is invariably produced when one heats even for a few minutes a thermometer which has not been previously heated for a long time to a temperature near that of the experiment. This movement can be effectually suppressed by preliminary processes of heating which will be described further on; and each series of depressions and reelevations must be repeated, in order to make sure that no permanent displacement of the zero has taken place. Let us suppose this preparation completed: the ther-

mometer has been heated to the highest degree of temperature employed in the experiments, 355° for instance; the position of the zero has been observed, the thermometer submitted to any series of operations; and, on heating again to 355°, the original position of the zero has been recovered; it remains to raise this depressed zero to its highest position. A thermometer can be kept indefinitely at the ordinary temperature, or even be heated for several weeks to 100°, without producing the total reelevation of the zero; and the study in detail of the means which facilitate that movement has revealed new facts of some importance in regard to the theoretic views considered in some preceding communications.

I will briefly repeat the explanation which seems to account best for all the movements of the fixed points of a thermometer. The glass, softened during the process of blowing, retains indefinitely, at the ordinary temperature, a residual separation of its particles, similar to that observed in an exaggerated degree in Rupert's drops. On such glass being heated, greater mobility is imparted to its particles, and the normal shrinking is induced (the disappearance of the abnormal expansion); and this action of heat is the more pronounced the more nearly the temperature is approached which produced the original separation. When a thermometer is in operation, the shrinkage causes a permanent raising of the zero-point, which may reach the considerable figure of 26°. It is evident that if a separation of the same kind, although inferior in amplitude, can be produced at will, can be made to persist for an indefinite time at the ordinary temperature, and to disappear by heating, this manner of understanding the phenomenon will be notably confirmed. Experiment realizes this prevision. In fact it is sufficient to know the true depression of the zero produced by boiling the mercury in a thermometer to ascertain that it persists indefinitely at the ordinary temperature. The depression is about 2° for French glass containing oxide of lead, the reelevation at the end of the first six months is from 0°·4 to 0°·5; the subsequent elevation during from five to ten years rarely exceeds 0°·5; and after ten years the zero remains stationary, leaving half of the depression subsisting, of which the total removal, equalling an additional degree, can be effected by suitable means. This conclusion has been confirmed by numerous experiments on the depressions produced at different temperatures. It is especially by heating to very high temperatures that depressions are produced which do not disappear. This phenomenon resembles that of the permanent expansion of a body when the limit of its elasticity has been passed.

The total reelevation after a depression of the zero, is accomplished more rapidly at high temperatures. Let us compare the periods of time when the intervals of temperature remain nearly equal. Five thermometers, after being heated for 24 hours to 306°, were kept at 218° until the position of the zero had become constant; for which four days were required. Afterwards the zero depressed at 218° was reelevated by heating for 18 days to 100°. It requires from six months to two years for the total

reelevation of the zero depressed at 100° when the thermometer is left at the ordinary temperature. The termination of the reelevation being the limit of the abatement of a movement, it is evident that it cannot be stated with exact precision; and it is especially by comparing the course at its different phases for each interval that one gets an exact idea of the variation of rapidity of the reelevation according to the temperature at which one works. When the intervals are smaller the reelevation is still more rapid: thus, from 0·8 to 0·9 of the total reelevation of the zero depressed at 100° can be produced by heating the thermometer during three days to 80°, four days to 60°, and five days to 40°.

It will be seen that, the greater the interval between the temperature which has produced a depression and that at which the thermometer is maintained to accomplish the reelevation, the slower is the movement; and it may not be complete if the interval notably exceeds 100°. These data have been utilized in the following manner to prepare some thermometers for the study of the real depressions of the zero. In order to cause the abnormal tension produced during the blowing of the glass to disappear, the instruments nos. 1 and 5 were heated 11 days to 355°; nos. 13 and 15 were heated three years to 206° and 266°; nos. 31 and 32, before being filled with mercury, were heated 100 hours to 440°, and cooled as gradually as possible during 100 hours; and finally all were heated for one day to 306° and the zero-points observed. In order to reelevate completely the zero from its position depressed by the heating to 306°, all the thermometers were heated and kept for four days at 218°, eighteen days at 100°, five days at 80°, seven days at 60°, six days at 40°, nine days at 20°, three days at 10°, and two days at zero.

Here only one series of experiments can be cited; they are summed up in the following Table: the first column contains the numbers of the thermometers; those which follow give the depressions corresponding to the temperatures indicated at the top of the columns.

	40°	60°	80°	100°	160°	218°	260°	306°	355°
1.....	0·00	0·06	0·19	0·31	0·74	1·12	1·33	1·63	2·19
5.....	0·04	0·08	0·18	0·29	0·56	0·76	0·91	1·14	1·51
13.....	0·02	0·03	0·17	0·31	0·69	0·87	1·09	1·30	2·15
15.....	0·01	0·05	0·18	0·31	0·75	0·97	1·12	1·40	2·05
31.....	0·02	0·06	0·22	0·37	0·84	1·15	1·46	1·77	..
32.....	..	..	..	0·28	0·69	0·98	1·21	1·56	2·06

Neglecting thermometer no. 5, which is of German soda glass, the numbers for all the others, which are of glass containing oxide of lead, are sufficiently concordant to permit the table to be employed for estimating, with but a very slight error, the depressions which will be produced by heating thermometers manufactured at Paris. The relation between the temperatures and the depressions might be expressed by a general formula; but simple interpolation suffices for practical needs.—*Comptes Rendus de l'Académie des Sciences*, May 8, 1882, t. xciv. pp. 1298–1301.

ON THE OSCILLATIONS OF THE PLANE OF POLARIZATION PRODUCED BY THE DISCHARGE OF A BATTERY: SIMULTANEITY OF THE ELECTRICAL AND OPTICAL PHENOMENA. BY D. BICHAT AND R. BLONDLOT.

We proposed to ourselves to study the rotation of the plane of polarization in a transparent body under the action of the current from the discharge of a Leyden jar. The experiment was arranged as follows.

Between a polarizer and an analyzer at extinction the transparent body is placed in a bobbin with a long and fine wire which is connected with the armatures of a battery. An exciter intercalated in the circuit permits the discharge to be produced when the difference of potential is sufficient. At the instant of each discharge the eye, placed in front of the analyzer, perceives a vivid reappearance of light, which shows that the plane of polarization has been deflected.

This fact having been proved, we sought to analyze it. For that purpose we placed before the optical apparatus a mirror rotating about a vertical axis. The polarizer was furnished with a slit, likewise vertical, the image of which was observed in the mirror by means of a telescope. By a suitable arrangement we compelled the spark to burst forth at the moment when the mirror, in its continuous rotation, occupied such a position that the image of the slit was visible in the telescope. We thus saw in the rotating mirror the reappearance of light due to each discharge.

What we observed was this:—In general, in the field of the telescope a series of broad luminous bands are seen, separated by narrower dark bands. The appearance reminds one absolutely of that which is observed when the light of the spark is examined. It is known that in that case the successive luminous bands correspond to currents alternately in opposite directions: the discharge is *oscillatory*. We have ascertained that it is the same with the plane of polarization. In fact, if the analyzer be rotated a small angle in a certain direction, the images of the even order are seen to be weakened, and at the same time the images of the odd order to increase in brightness. If the rotation take place in the opposite direction, the images of the odd order are weakened, and those of the even order become brighter.

The plane of polarization, then, undergoes successive rotations alternately in opposite directions; it *oscillates* about its normal position. To each oscillatory discharge corresponds an oscillatory movement of the plane of polarization.

This being admitted, is there simultaneity between the electric and optic phenomena? or does the movement of the plane of polarization manifest itself in an appreciable time after the electric action? We have solved this question in the following manner:—To the apparatus employed for the preceding experiments we added an arrangement permitting to be seen at the same time, in the rotating mirror, the bands furnished by the light of the spark and those due to the oscillation of the plane of polarization. For this

purpose the exciter was placed so that the spark, by means of a suitable optical system, illuminated a vertical slit. A fixed mirror sent back the light proceeding from this slit upon the rotating mirror, and thence into the telescope. The movable mirror being at rest, the vertical images of the two slits were seen in the field distinctly. By regulating the position of the fixed mirror, these two slits, which had the same breadth, were brought to be each precisely in the prolongation of the other.

During the rotation of the mirror, at the moment when the battery is discharged, each of the images dilates in the horizontal direction. Two systems of bands alternately luminous and dark are thus seen one above the other: the one is due to the light of the spark; the other proceeds from the polarizing-apparatus.

The experiment shows that the brilliant bands of one of the systems forms exactly the prolongation of the luminous bands of the other, and that it is the same with the dark bands. If the mirror be rotated more and more rapidly, the breadth of the bands increases, *but the correspondenee of the two systems still remains perfect.* Therefore, with the close approximation which our apparatus permits us to obtain, we may conclude that *the two phenomena, electrical and optical, are simultaneous.*

In order to measure that approximation, we slightly displaced the fixed mirror, so as to destroy the correspondence of the two images. It is clear that the displacement produced the same effect as any delay that might have existed between the two orders of phenomena. We thus secured that a delay of $\frac{1}{30000}$ of a second should be quite appreciable; we can therefore affirm that the delay, if it exists, is less than $\frac{1}{30000}$ of a second.

The experiments were made with heavy flint glass and bisulphide of carbon successively as the transparent body. M. Villari, by causing a cylinder of flint glass to rotate between the poles of an electromagnet*, ascertained that at a sufficient velocity the phenomenon of rotatory polarization ceases to exist; hence he concluded that, to produce magnetization of the flint glass, a time comprised between 0·001244 and 0·00241 of a second is required. Now the sensitiveness of our method permitted us to appreciate a displacement corresponding to a forty-fourth part of that time.

An unpublished experiment of MM. P. Curie and Ledeboer accords with our conclusions. Substituting for the copper disk of Foucault's apparatus a glass disk, and causing it to rotate at the rate of a hundred turns per second, they observed no diminution in the rotation of the plane of polarization. It seems, then, that another explanation of the very interesting experiment of M. Villari must be sought.

We are moreover in accord with him upon this point, that the rotation of the plane of polarization ceases at the same instant as the electric action.—*Comptes Rendus de l'Académie des Sciences*, June 12, 1882, t. xciv. pp. 1590–1592.

* Pogg. *Ann*, cxlix. p. 324 (1873).

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JOURNAL OF SCIENCE.

[FIFTH SERIES.]

AUGUST 1882.

X. *Remarks on Absolute Systems of Physical Units.* *By* A. F. SUNDELL*.

THE great importance of the absolute system of physical units, introduced by Gauss, and developed by William Weber, Kohlrausch, Maxwell, and Jenkin, is no doubt fully recognized by every physicist. It is therefore the more surprising that the so-called *dimensions* of the physical units are so little employed in practice, notwithstanding the demonstration of their great utility given by Prof. Kohlrausch in his well-known *Leitfaden der praktischen Physik*. These dimensions may be regarded as the actual *names* of the units; and where they do not accompany the statement of the numerical values of magnitudes, the data are as incomplete as if, in giving a length or a surface, the unit of length or of surface employed were not mentioned.

The works of the authors named contain all that is necessary for the practical employment of the absolute system. But inasmuch as there are several different systems of absolute units employed in physics whose mutual relationships have not been sufficiently clearly pointed out, although definite indications of the connexion between them are to be found, especially in Weber's *Elektrodynamische Maassbestimmungen*, and in Maxwell's great work, 'A Treatise on Electricity and Magnetism,' I propose to put forward here an elementary theory of absolute systems of physical units.

* Translated from a separate impression from the *Acta Soc. Scient. Fenn.* Tom. xii. (Helsingfors, 1881), communicated by the Author.

1. Various physical conceptions have resulted from the endeavour of physicists to represent the results of their researches mathematically as simply as possible, and to extend them. Most physical conceptions, even if not required for this special purpose, are at any rate absolutely necessary for a satisfactory formal demonstration of physical laws. If, for example, we seek to represent the law of a given uniform motion mathematically, the equation

$$s=ct \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is sufficient; in which s represents the distance described in the time t, and c is a constant dependent on the units employed for s and t. If by means of this equation we determine the value of the constant c by means of the distance described in a given time, we are then able to calculate the distance for any time t. The equation (1) may then be regarded as the mathematical representation of the law of the given motion. But with the value found for the constant, the equation can be used only for this particular motion, and not for any other uniform motion. But for any other motion of the same kind, an equation of the same form may be used, but with a different value of the constant c. It is in fact impossible to give a general formula for uniform motion with only the two conceptions of *distance* and *time*. But if we employ also a third conception, h, proportional to the ratio of *distance* to *time*, we obtain the formula

$$h=c\frac{s}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is sufficient for any uniform motion. This conception (h) is of course that of velocity. If from any particular uniform motion we determine the value of the constant c, we are able to calculate from the formula (2) the value of any one of the three quantities when the values of the two other quantities are given.

Equation (2) may be regarded as the general formula of uniform motion; but it may also be regarded, on the other hand, as an equation defining the conception of velocity, if we take the conceptions of space and time as already in any way defined. Each new conception in physics may be defined by an equation containing this new quantity, together with other quantities previously defined. Thus, for example, the conception of acceleration may be defined by the equation

$$a=c\frac{s}{t^2},$$

which relates to uniformly accelerated motion with zero initial

velocity. But since $\frac{s}{t}$ is proportional to a certain velocity (viz. the final velocity), this equation may be better written

$$a=c\frac{h}{t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In order to extend these conceptions to every case, we must often take account of infinitesimals. Thus, for example, to extend the conception of velocity to the case of motion not uniform, it must be defined by the differential equation

$$h=c\frac{ds}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Acceleration, when not uniform, is defined by the equation

$$a=c\frac{dh}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

But even in these new forms the equations express the same things as in their simpler form. Velocity is proportional to the increase of distance, and acceleration to the increase of velocity in the unit time. The conceptions of force (k) and mass (m) are connected by the equation

$$k=cma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This equation may be regarded as a definition of force, if mass be taken as a conception previously defined—or, on the other hand, as a definition of mass, if the conception of force has been first defined in any way. Each additional conception is defined by a separate equation. Moreover, these quantities may be connected by equations distinct from the defining equations which express natural laws. Thus, for example, the Newtonian law of gravitation gives the equation connecting length, mass, and force,

$$k=c\frac{m^2}{\mathrm{L}^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where k denotes the force with which a mass m attracts an equal mass at a distance L.

2. We have thus a certain number (p) of distinct equations in which q quantities occur. Each equation may contain a constant. The units of the different quantities may be chosen at pleasure; the numerical value of the constant in an equation depends on the units of the quantities occurring in the equation. If, for example, we determine the constant of equation (2) from the velocity of light, and take the geographical mile as unit of length, the second as unit of time, and the

velocity of light as the unit of velocity, we must put $s=40000$, $t=1$, $h=1$; hence $c=\frac{1}{40000}$. Equation (2) becomes therefore $h=\frac{s}{40000t}$, or $s=40000\,ht$; which holds good for any uniform motion so long as distance, time, and velocity are expressed in the units chosen. But if we take as unit of length a quadrant of the earth's meridian (1350 geographical miles) without changing the other units, then we must put $s=\frac{40000}{1350}=\frac{800}{27}$, and the constant c has now the value $\frac{27}{800}$.

As the constants of the equations thus change with the units, so, conversely, by choosing appropriate units, particular values may be given to the constants. If we choose the units so as to make certain constants equal to unity, the corresponding equations assume the simplest form, and the constants disappear from them. Units so chosen are termed *absolute units*, and form an *absolute system*. If we assume that p (the number of equations) is smaller than q (the number of quantities), then we may eliminate all the constants by choosing the units properly; $q-p$ units remaining undetermined, and p units having values determined by the condition that each constant is equal to unity. But if we wish to have more than $q-p$ units, say $q-p+x$ undetermined, then x constants must be retained, which are then to be considered as independent conceptions. Let us take a few examples. The four conceptions distance, time, velocity, and acceleration are connected by the equations (2) and (3). If we wish to eliminate the two constants, we must take as unit of velocity the velocity of the uniform motion in which unit distance is described in unit time; the unit of acceleration must be the acceleration of a uniformly accelerated motion in which the velocity increases or diminishes in unit time by unit velocity.

Then equations (2) and (3) may be written as follows:—

$$h=\frac{s}{t}, \quad \dots\dots\dots \quad (8)$$

$$a=\frac{h}{t}; \quad \dots\dots\dots \quad (9)$$

and similarly equations (4) and (5) become

$$h=\frac{ds}{dt}, \quad \dots\dots\dots \quad (10)$$

$$a=\frac{dh}{dt}. \quad \dots\dots\dots \quad (11)$$

Of the four units, any two are chosen at pleasure. If we choose for units of length and time the metre and the second respectively, then the unit of velocity becomes the velocity of a uniform motion in which 1 metre is described in 1 second; and if the velocity of uniformly accelerated motion change by one unit of velocity in one second, then its acceleration is unity. But if we arbitrarily choose the second and the velocity of light as units, then, according to equation (8), the unit of length is the distance (40,000 geographical miles) which light traverses in one second. The corresponding acceleration will be, as we easily see, $\frac{40000 \times 7420}{9 \cdot 81}$ times as great as the acceleration due to gravity. We need not follow out the two other possible combinations.

By choosing suitable units of mass and force we may eliminate the constants of equation (6), which assumes the form

$$k = ma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

The six quantities—space, time, velocity, acceleration, force, and mass—are connected by the four equations (2), (3), (6), and (7); if we wish to eliminate the four constants, we must retain the four units already determined. Equation (7) assumes then the form

$$k = \frac{m^2}{L^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

The units of mass and force must be chosen so that, on the one hand (equation 12), unit force communicates unit acceleration to unit mass, and so that, on the other hand, unit mass attracts an equal mass at unit distance with unit force. Of the six units any two may be chosen at pleasure. But if we wish to choose three units at pleasure—say those of length, time, and mass—then we must retain one constant, for example that of equation (7) (the "constant of attraction"); for, since the units have been some of them chosen at pleasure, and some of them determined by means of the three remaining equations, the constant of the equation in question does not usually become unity. We may then regard this constant as a quantity defined by the equation

$$c = \frac{kL^2}{m^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

If we wish to choose four units at pleasure, say those of length, time, force, and mass, we must retain one more constant, for example that of equation (6).

By the introduction of absolute units the equations of defi-

nition are much simplified; since the constants of the equations are partly eliminated, partly regarded as new conceptions, it may be said that the equations from which an absolute system is derived contain no constants—that is, no numerical factors; each side of the equation has only one term. We will call these equations the fundamental equations. Together with these equations there occur in physics a large number of equations or formulæ; but these are not distinct from the fundamental equations, but are formed from them by various combinations and methods of calculation, integration, and so forth: they are therefore not so simple as the fundamental equations; the two sides may contain several terms, into which various numerical factors enter. It is necessary in our choice of units not to lose sight of these equations derived from the fundamental equations; only, in using them, we must express the quantities in the units obtained from the corresponding fundamental equations. If, for example, we employ the formula for centrifugal force,

$$k=\frac{mh^2}{r},$$

we must express k, m, h, and the radius of curvature r in the units which correspond to the fundamental equations (8), (9), and (12).

In the same way, the general equation for uniformly accelerated motion,

$$s=h_0t+\tfrac{1}{2}at^2,$$

requires the units for s, t, h, h_0, and a, which are obtained from the fundamental equations (8) and (9).

3. According to what has been explained, in an absolute system certain units may be chosen at pleasure; these are termed *fundamental units*. The magnitudes of all the other units are determined by the condition that the constants in certain equations of definition are to be equal to unity: such units are therefore termed *derived units**. We see that, in consequence of the mode in which the absolute units are determined, the fundamental equations will be satisfied if in them we replace the quantities by their units. If, for example, we take equation (8) for velocity, it will also hold good if we replace the quantities by their respective units. But if we do this in the ordinary way, by putting $h=1$, $s=1$, and $t=1$, we arrive at the identity $1=1$, which is of no further use to us. But if we put $s=$ unit length, $t=$ unit time, $h=$ unit velocity,

* Kohlrausch, *Leitfaden*, 3rd ed. p. 206; Maxwell, 'Treatise on Electricity and Magnetism,' i. pp. 2 & 5.

we obtain the relation

$$\text{unit velocity} = \frac{\text{unit length}}{\text{unit time}};$$

i. e. unit velocity and the quotient $\frac{\text{unit length}}{\text{unit time}}$ are identical magnitudes; and we may therefore regard this quotient as the *name* of the unit of velocity, if this unit is derived from the units of length and time. But if we take the metre as the unit of length and the second as the unit of time, then the velocity derived from these units must be called $\frac{\text{metre}}{\text{second}}$, which means that a velocity equal to 10 such units of velocity is to be written $10\,\frac{\text{metre}}{\text{second}}$.

The function $\frac{\text{unit length}}{\text{unit time}}$ is called by Maxwell* the *dimensions* of the unit of velocity. If in all the fundamental equations we replace quantities by their units, we obtain a system of equations according to which the absolute units are functions amongst themselves of certain fundamental units. These functions, which are obtained by solution of these last-named equations, are the *dimensions* or *names* of the derived units.

For the sake of brevity we will, as a rule, denote the units by the same symbols as the quantities; but, to avoid confusion, we will include the symbols of units in brackets. Thus $[l]$ will denote the unit of length.

As an example of the different systems of units and dimensions, we will consider the group of the six conceptions of length, time, velocity, acceleration, mass, and force. We will choose $[l]$, $[t]$, and $[m]$ as fundamental units; then from the four equations (2), (3), (6), (7) we may omit three constants, say those of the first three equations; and so we obtain as fundamental equations the equations (8), (9), (12), (7). By substitution of the units we obtain the following relations:—

$$[h]=\left[\frac{l}{t}\right], \quad]a]=\left[\frac{h}{t}\right], \quad [k]=[ma], \quad [k]=\left[c_1\frac{m^2}{l^2}\right]. \qquad (15)$$

By solution of these equations we obtain the following dimensions:—

$$[h]=\left[\frac{l}{t}\right], \quad [a]=\left[\frac{l}{t^2}\right], \quad [k]=\left[\frac{ml}{t^2}\right], \quad [c_1]=\left[\frac{l^3}{t^2m}\right]^{1}. \qquad (16)$$

If we keep the same fundamental equations, but choose $[l]$,

* 'Treatise on Electricity and Magnetism,' i. p. 2. Compare Kohlrausch, *Leitfaden*, p. 207.

$[t]$, and $[k]$ for fundamental units, the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [m]=\left[\frac{t^2k}{l}\right],\quad [c_1]=\left[\frac{l^4}{t^4k}\right]. \quad (17)$$

If, on the other hand, we leave the constant of equation (6) standing, we obtain new fundamental equations (8), (9), (6), and (13) for a system with three fundamental units, and the equations of the units become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{h}{t}\right],\quad [k]=[c_2ma],\quad [k]=\left[\frac{m^2}{l^2}\right], \quad (18)$$

where c_2 denotes the constant of equation (6). If, again, we choose $[l]$, $[t]$, and $[m]$ as fundamental units, we obtain the dimensions

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [k]=\left[\frac{m^2}{l^2}\right],\quad [c_2]=\left[\frac{mt^2}{l^3}\right]. \quad (19)$$

We see that the dimensions of units change not only with change of the fundamental units, but also when the fundamental units remain the same and the fundamental equations are taken differently.

If we wish to regard only $[l]$ and $[t]$ as fundamental units, all four constants are eliminated; the equations (8), (9), (12), and (13) become fundamental equations, and the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [m]=\left[\frac{l^3}{t^2}\right],\ \text{and}\ [k]=\left[\frac{l^4}{t^4}\right]. \quad (20)$$

On the other hand, if we assume four fundamental units, we must leave two constants standing. If we take equations (8), (9), (6), and (7) as fundamental equations, the equations of units become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{h}{t}\right],\quad [k]=[c_2ma],\quad [k]=\left[c_1\frac{m^2}{l^2}\right]. \quad (21)$$

If $[l]$, $[t]$, $[m]$, and $[k]$ are the fundamental units, the dimensions become

$$[h]=\left[\frac{l}{t}\right],\quad [a]=\left[\frac{l}{t^2}\right],\quad [c_2]=\left[\frac{t^2k}{lm}\right],\quad [c_1]=\left[\frac{l^2k}{m^2}\right]. \quad (22)$$

The choice of fundamental units is limited only by the consideration that they must not all occur in the same formula. Hence we see from equation (15) or (16) that $[l]$ $[t]$ and $[h]$, $[h]$ $[a]$ and $[t]$, $[l]$ $[t]$ and $[a]$, or $[k]$ $[m]$ and $[a]$ cannot be fundamental units at one and the same time; on the

other hand, the last-mentioned group may be employed for this purpose in the system corresponding to equation (8).

4. It is very important to be able to pass from one system of units to another, and to determine the ratio between the units of the two systems. This problem presents the following special cases:—

I. When the new system contains one new fundamental unit besides the fundamental units of the old system. Let L be the value in the old system of the new fundamental unit defined by the equation

$$L = M^h N^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

where M and N are quantities defined independently of L. As the unit for L in the new system may be chosen at pleasure, this quantity is expressed by a new number L_1; but the numerical values of M and N are not altered. Equation (23) is therefore not satisfied by L, M, and N, but must be written with a constant α,

$$L_1 = \alpha M^h N^k. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

The system of fundamental equations is therefore changed: the number of equations remains the same; but the number of quantities is increased by one. Since we must regard α as a new conception defined by equation (24), we must therefore increase the number of fundamental units by one. Consequently the relations of the units as well as their dimensions are partially altered. By comparing equations (23) and (24) we find that

$$L = \frac{L_1}{\alpha}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

whence, since the units are inversely proportional to the numerical values of a given quantity, it follows that

$$[L] = n[L_1], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25a)$$

where n is the numerical value of α.

The ratios of the remaining units are easily obtained from the other fundamental equations without alteration. It is to be remarked that the constant α is in general not an abstract number, but that the dimensions of its unit are given by the equation

$$[L_1] = [\alpha M^h N^k]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

If, for example, we wish to pass from the system (15) into the system (21), where, besides the three fundamental units $[l]$, $[t]$, and $[m]$, we take $[k]$ also as a fundamental unit, we must start from the equation (12), which defines the conception k.

This equation becomes

$$k_1=c_2 ma.$$

We have between the old unit of force and the new one the relation

$$[k]=n[k_1],$$

where n denotes the numerical value of c_2. The new dimensions of the units are determined by equation (22).

Example 1.—Let us take as the old unit of force

$$[k]=1\,\frac{\text{metre}\times\text{kilogramme}}{(\text{second})^2},$$

and as new unit of force the weight of a gramme at the Observatory in Paris; then, by equation (2), since $m=0{\cdot}001$ kilogramme, and $a=g=9{\cdot}808\,\frac{\text{metre}}{(\text{second})^2}$,

$$[k_1]=0{\cdot}009808\,\frac{\text{metre}\times\text{kilogramme}}{(\text{second})^2}=0{\cdot}009808[k].$$

Hence $n=\frac{1}{0{\cdot}009808}$; and as the dimensions of c_2 are $\left[\frac{t^2k_1}{lm}\right]$, the complete value of the new constant becomes

$$c_2=\frac{1}{0{\cdot}009808}\,\frac{(\text{second})^2\times\text{gramme (Paris)}}{\text{metre}\times\text{kilogramme}},$$

if we call the new unit of force a gramme (Paris). We have further,

$$\left.\begin{aligned}&1\text{ gramme (Paris)}=0{\cdot}009808\,\frac{\text{metre}\times\text{kilogr.}}{(\text{second})^2},\\&1\,\frac{\text{metre}\times\text{kilog.}}{(\text{second})^2}=\frac{1}{0{\cdot}009808}\text{ gramme (Paris)}.\end{aligned}\right\}\quad(27)$$

II. One of the fundamental units of the old system is a derived unit in the new system; the remaining units are common to both systems. Since the new system contains one fundamental unit less, it must also contain one constant less. The new system is therefore obtained by eliminating a constant.

Let

$$\alpha\mathrm{L}=\mathrm{M}^h\,\mathrm{N}^k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

be the equation containing the constant α to be eliminated. In the new system this equation assumes the form

$$\mathrm{L}_1=\mathrm{M}^h\mathrm{N}^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

and defines the conception L_1; the dimensions of its unit are

given by the equation

$$[L_1] = [M^h N^k]. \qquad (30)$$

Now we find

$$\alpha L = L_1,$$

$$[L] = n[L_1], \qquad (31)$$

where, again, n is the numerical value of α. If we introduce into this equation the dimensions of $[L_1]$ as well as the dimensions of $[L]$, if this unit is also a derived unit in the old system, we obtain at any rate a relation into which the fundamental unit enters, which is to be made a derived unit. We can then determine from equation (31) the ratio of the units in question in the old and new systems.

If, for example, we wish to pass from the last system with four fundamental units to system (17), where $[l]$, $[t]$, and $[k]$ are fundamental units, we must make $[m]$ a derived unit and eliminate the constant c_2 from the equation $k = c_2 ma$. We accomplish this by putting $c_2 m = m_1$, whence

$$[m] = n[m_1],$$

where n is the numerical value of c_2. The new unit of mass $[m_1]$, according to equation (17), has the dimensions $\left[\frac{t^2 k}{l}\right]$. If we retain the last units, we have

$$c_2 = \frac{1}{0{\cdot}009808} \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}},$$

and

$$\left.\begin{aligned} 1 \text{ kilogramme} &= \frac{1}{0{\cdot}009808} \frac{\text{sec.}^2 \times \text{gramme (Paris)},}{\text{metre}} \\ 1 \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre}} &= 0{\cdot}009808 \text{ kilogramme.} \end{aligned}\right\} \qquad (32)$$

If we wish to make $[t]$ and not $[m]$ a derived unit, we must put $c_2 a = a_1$, so that the equation $k = c_2 ma$ becomes $k = ma_1$, defining the conception a_1.

We obtain then

$$[a] = n[a_1];$$

and by introducing the dimensions of $[a]$ and $[a_1]$,

$$\left[\frac{l}{t^2}\right] = n\left[\frac{k}{m}\right],$$

whence

$$[t] = \frac{1}{\sqrt{n}}\left[\frac{l^{\frac{1}{2}} m^{\frac{1}{2}}}{k^{\frac{1}{2}}}\right] = \frac{1}{\sqrt{n}}[t_1].$$

Retaining the former units, we have

$$\left.\begin{aligned} 1 \text{ second} &= \sqrt{0{\cdot}009808}\,\frac{\text{metre}^{\frac{1}{2}}\ \text{kilogr.}^{\frac{1}{2}}}{\text{gramme (Paris)}^{\frac{1}{2}}}, \\ 1\,\frac{\text{metre}^{\frac{1}{2}}\times\text{kilogr.}^{\frac{1}{2}}}{\text{gramme(Paris)}^{\frac{1}{2}}} &= \frac{1}{\sqrt{0{\cdot}009808}}\ \text{second.} \end{aligned}\right\} \quad . \quad (33)$$

In the same way we find, if $[l]$ is to be made a derived unit,

$$\left.\begin{aligned} 1 \text{ metre} &= \frac{1}{0{\cdot}009808}\,\frac{\text{gramme (Paris)}\times\text{sec.}^2}{\text{kilogr.}}, \\ 1\,\frac{\text{gramme (Paris)}\times\text{sec.}^2}{\text{kilogr.}} &= 0{\cdot}009808 \text{ metre.} \end{aligned}\right\} . \, (34)$$

It is to be remarked that the relations (27), (32), (33), and (34) may be regarded as determinations of the four units from any one of them.

III. By successive employment of the two preceding methods we are able to exchange one fundamental unit for another, while retaining the same fundamental equations. In one of the examples given, we have in fact exchanged the unit of mass (kilogramme) for the unit of force (gramme, Paris) while retaining the two remaining units (metre and second).

IV. By successively employing methods I. and II., we may change the system of fundamental equations by transferring a constant from one equation to another without altering the number of the fundamental units. If, for example, we wish to pass from the system (15) to the system (18), we first of all eliminate the constant of equation (7) by method (II.), and then introduce a constant into equation (12) by method I. We may of course adopt the reverse method. The calculation is most simple if it is possible to retain the same fundamental units. If, for example, in the system of equations (8), (9), (12), and (7) the constant is to be transferred from equation (7) to equation (8), we must make the following substitutions, $[l]$, $[m]$, and $[t]$ being the fundamental units :—in equation (7), $\frac{k}{c}=k_1$; in equation (12), $k=k_1c$, and hence $\frac{a}{c}=a_1$; in equation (9), $a=a_1c$, and hence $\frac{h}{c}=h_1$; and in equation (8), $h=h_1c$. We obtain thus the fundamental equations

$$h_1=\frac{1}{c}\,\frac{s}{t}; \quad a_1=\frac{h_1}{t}; \quad k_1=ma_1; \quad k_1=\frac{m^2}{l^2};$$

and the dimensions become

$$[k_1]=\left[\frac{m^2}{l^2}\right],\quad [a_1]=\left[\frac{m}{l^2}\right],\quad [h_1]=\left[\frac{mt}{l^2}\right],\quad [c]=\left[\frac{l^3}{t^2m}\right],$$

as before.

5. The rules we have given make it possible to pass from one system of units to another. The process, however, is somewhat troublesome. When the magnitude of a physical conception is given in one system of units and we wish to find its numerical value in another system, the following is in all cases the safest and quickest method of arriving at the result*:—

If we assume three fundamental units and denote them by P, Q, R, then the dimensions of a derived unit N are of the form

$$N=P^aQ^bR^c,$$

where a, b, c are positive or negative rational exponents. If we assume units of other magnitudes p, q, r, and if $P=ep$, $Q=fq$, $R=gr$, where e, f, and g are constants, then

$$N=e^af^bg^c.p^aq^br^c.$$

If p, q, and r are fundamental units, the new derived unit n has the dimensions p^a, q^b, r^c, and

$$N=e^af^bg^c.n,$$

or the given unit N is $e^af^bg^c$ times as large as the new unit n.

If one of the new units, for example p, be itself a unit derived from the units q, r, and a new fundamental unit s, by means of the formula

$$p^a=q^{b_1}r^{c_1}s^d,$$

then

$$N=e^af^bg^c.p^aq^br^c=e^af^bg^c.q^{b+b_1}r^{c+c_1}s^d.$$

The new unit n is determined by the formula

$$n=q^{b+b_1}r^{c+c_1}s^d;$$

consequently in this case also

$$N=e^af^bg^c.n.$$

We obtain thus the following practical rule for obtaining the value of a unit referred to new fundamental units:—In the dimensions of the unit replace the old fundamental units by their values in the new fundamental units, and carry out the algebraic operations indicated as if the names of the units were algebraic magnitudes. This rule gives not only the ratio of the old unit N to the new unit n, but also the dimen-

* Compare Kohlrausch, *Leitfaden*, p. 207.

sions of the new unit. Of course the rule holds good if the number of fundamental units is greater or smaller than three.

Example 2.—The unit of force derived from the metre, second, and kilogramme $=1\,\frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}$. If now we take the centimetre, the gramme, and the minute as new fundamental units, we put

metre = 100 centimetres, kilogramme = 1000 grammes, second = $\frac{1}{60}$ minute,

and so obtain

$$1\,\frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}=\frac{100\text{ centimetres}\times 1000\text{ grammes}}{(\frac{1}{60}\text{ minute})^2}$$

$$=100\times 1000\times 60^2\,(=360\times 10^6)\,\frac{\text{centim.}\times\text{gramme}}{(\text{minute})^2};$$

that is, the first unit of force is 360 million times larger than the second.

If here we wish to exchange the former unit of mass, the kilogramme, for the derived unit $\frac{(\text{sec.})^2\times\text{gramme (Paris)}}{\text{metre}}$, then by equation (32) we obtain

$$1\,\frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}=1\,\frac{\text{metre}\times\frac{1}{0\cdot 009808}\times\frac{(\text{sec.})^2\times\text{grm. (Paris)}}{\text{metre}}}{(\text{sec.})^2}$$

$$=\frac{1}{0\cdot 009808}\text{ gramme (Paris)}.\text{ Compare equation (27).}$$

It is clear that the same rule holds for the reduction of any magnitudes to new fundamental units.

Example 3.

$$1\text{ Daniell}=112\times 10^9\,\frac{\text{millim.}^{\frac{3}{2}}\times\text{mgr.}^{\frac{1}{2}}}{\text{sec.}^2}\text{ (Gauss-Weber system)}$$

$$=112\times 10^9\,\frac{(0\cdot 001\text{ metre})^{\frac{3}{2}}\times(0\cdot 000001\text{ kilogr.})^{\frac{1}{2}}}{\text{sec.}^2}$$

$$=\frac{112\times 10^9}{10^{\frac{9}{2}}\times 10^{\frac{6}{2}}}\,\frac{\text{metre}^{\frac{3}{2}}\times\text{kilogr.}^{\frac{1}{2}}}{\text{sec.}^2}$$

$$=112\times 10^{\frac{3}{2}}\,\frac{\text{metre}^{\frac{3}{2}}\times\text{kilogr.}^{\frac{1}{2}}}{\text{sec.}^2}$$

$$=112\times 10^{\frac{3}{2}}\,\frac{\text{metre}^{\frac{3}{2}}\times\left(\frac{1}{0\cdot 009808}\,\frac{\text{sec.}^2\times\text{grm. (Paris)}}{\text{metre}}\right)^{\frac{1}{2}}}{\text{sec.}^2}$$

$$=\frac{112\times 10^{\frac{3}{2}}}{\sqrt{0\cdot 009808}}\,\frac{\text{metre}\times\text{gramme (Paris)}^{\frac{1}{2}}}{\text{sec.}}\text{ (gravitation system, Equation 32).}$$

By the aid of this rule, the elimination of a constant is more easily effected than as given above by II. If the constant to be eliminated has the value

$$\alpha = n[P^a Q^b R^c],$$

where [P], [Q], and [R] are the fundamental units, and if we wish to make the fundamental unit [P], for example, into a derived unit and at the same time to eliminate the constant, *i. e.* to make it equal to unity, we must put

$$[P] = n^{-\frac{1}{a}}[Q^{-\frac{b}{a}}\ R^{-\frac{c}{a}}],$$

where α becomes equal to unity.

If, again, we wish to eliminate the constant

$$c_2 = \frac{1}{0{\cdot}009808}\ \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}$$

introduced into example 1 by making the unit of mass a derived unit, we do so by putting

$$1 \text{ kilogramme} = \frac{1}{0{\cdot}009808}\ \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre}}$$

(compare equation 32); c_2 becomes equal to unity, and we have a system in which the only fundamental units are the units of length, time, and force. In order to make the unit of time a derived unit, we put

$$1 \text{ sec.}^2 = 0{\cdot}009808\,\frac{\text{metre} \times \text{kilogr.}}{\text{grm. (Paris)}}$$

or

$$1 \text{ sec.} = \sqrt{0{\cdot}009808}\ \frac{\text{metre}^{\frac{1}{2}}\ \text{kilogr.}^{\frac{1}{2}}}{\text{grm.}^{\frac{1}{2}}\ \text{(Paris)}}$$

(compare equation 33), and so on.

We may follow a similar rule in using a physical formula to find the value of an unknown magnitude. We introduce the numerical values of the known magnitudes into the formula, together with the appropriate dimensions or names of the units, and solve the equation, thus finding at once the value of the unknown magnitude and its dimensions.

Example 4.—Let us determine the value of the constant c_2 from the equation $k_1 = c_2 ma$ (compare example 1). If the force $k_1 = 1$ gramme (Paris) act upon the mass $m = 0{\cdot}001$ kilogramme, it produces the acceleration

$$a = g \text{ (Paris) } 9{\cdot}808\ \frac{\text{metre}}{\text{second}^2}.$$

If we put these values in the equation given, we have

$$1 \text{ gramme (Paris)} = c_2 \times 0{\cdot}001 \text{ kilogramme} \times 9{\cdot}808 \frac{\text{metre}}{\text{sec.}^2},$$

$$1 \text{ gramme (Paris)} = 0{\cdot}009808 \frac{\text{metre} \times \text{kilogramme}}{\text{sec.}^2} \times c^2,$$

and

$$c_2 = \frac{1}{0{\cdot}009808} \frac{\text{sec.}^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}.$$

The rule given above is also of service in the exchange of units, as is shown in the following examples:—

Example 5.—Let the velocity of light be taken as fundamental unit instead of the unit of time. We know that

$$\text{Velocity of light} = 40{,}000 \frac{\text{geographical miles}}{\text{second}}.$$

Then we have the velocity

$$1 \frac{\text{metre}}{\text{second}} = \frac{1}{7420} \frac{\text{geogr. mile}}{\text{second}} = \frac{1}{7420 \times 40{,}000} \text{ vel. of light},$$

and the acceleration

$$9{\cdot}808 \frac{\text{metre}}{(\text{sec.})^2} = 9{\cdot}808 \frac{(\text{metre})^2}{(\text{sec.})^2} \times \frac{1}{\text{metre}}$$

$$= \frac{9{\cdot}808}{7420^2 \times 40{,}000^2} \frac{(\text{velocity of light})^2}{\text{metre}}.$$

From this last number we may obtain the ordinary value as follows:—

$$\frac{9{\cdot}808}{7420^2 \times 40{,}000^2} \frac{\text{vel. of light}^2}{\text{metre}} = \frac{9{\cdot}808 \times 40{,}000^2}{7420^2 \times 40{,}000^2} \frac{\text{geogr. mile}^2}{\text{sec.}^2 \times \text{metre}}$$

$$= \frac{9{\cdot}808 \times 40{,}000^2 \times 7420^2}{7420^2 \times 40{,}000^2} = 9{\cdot}808 \frac{\text{metre}}{\text{sec.}^2}.$$

Example 6.—Let the acceleration due to gravity (Paris) $= 9{\cdot}808 \frac{\text{metre}}{\text{sec.}^2}$ be taken as the fundamental unit instead of the unit of time. We obtain then, for example,

$$1 \text{ horse-power} = 75 \frac{\text{metre} \times \text{kilogr. (Paris)}}{\text{second}} = 75 \frac{\text{met.}^{\frac{1}{2}}}{\text{sec.}} \times \text{metre}^{\frac{1}{2}}$$

$$\times \text{kilogr.(Paris)} = 75\left(\frac{\text{met.}}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \text{met.}^{\frac{1}{2}} \times \text{kilogr.(Paris)}.$$

$$= \frac{75}{\sqrt{9{\cdot}808}} \text{ acceleration due to gravity (Paris)}^{\frac{1}{2}} \times \text{metre}^{\frac{1}{2}} \times \text{kilogr. (Paris)}.$$

6. The systems of units most frequently employed in physics are that due to Gauss and Wilhelm Weber, whose fundamental units are those of length, time, and mass, and that employed in many text-books (*e. g.* in Wüllner's *Experimental-Physik*), as well as by engineers, which has for fundamental units the units of length, time, and force. In this last system the gravitating force of a unit of weight is taken as the unit of force. This system may therefore be termed the gravitation system. The Newtonian equation (7) holds good for this system with the introduction of a constant (constant of attraction). The examples given above (examples 2 and 3) show how a quantity given in the Gauss-Weber system may be transformed into the gravitation system: the unit of mass of the first system must in general be replaced by the derived unit of mass of the second system. Conversely, if we wish to pass from the second system to the first, we must replace the unit of force by the derived unit of force of the first system. For this purpose we employ relationships of the same kind as equations (32) and (27).

The reduction is made most simply by employing the gravitating force of the unit of mass (m) of the first system as the unit of force (k_1) of the second system. We have then

$$\left.\begin{aligned} & [k_1] = g[k] = g\left[\frac{lm}{t^2}\right], \\ \text{and hence} \quad & [m] = \frac{1}{g}\left[\frac{t^2 k_1}{l}\right], \end{aligned}\right\} \quad \ldots \ldots \quad (35)$$

where g expresses the numerical value of the acceleration of gravity at the place. Since the name of unit of weight is employed both for the unit of mass and for the unit of force, these units may be easily distinguished from each other by attaching to the name of the unit of force the name of the place where the gravitating force of the unit of weight is equal to the unit of force, as we have done in previous examples.

Example 7.

$$\text{One horse-power} = 75\,\frac{\text{metre} \times \text{kilogramme (Paris)}}{\text{second}}$$

(gravitation system)

$$= 75\,\frac{\text{metre} \times 9{\cdot}808\,\dfrac{\text{metre} \times \text{kilogr.}}{\text{second}^2}}{\text{second}}$$

$$= 75 \times 9{\cdot}808 (= 735{\cdot}6)\,\frac{\text{metre}^2 \times \text{kilogr.}}{\text{second}^2}$$

(Gauss-Weber system).

The chief constant of the two systems is the constant of

attraction—which is equal to the square of the so-called Gauss's constant. The value of this constant is given by Gauss* as

$$0{\cdot}0172\,\frac{\text{semi- major axis of earth's orbit}^{\frac{3}{2}}}{\text{mean day}\ \times\ \text{sun's mass}^{\frac{1}{2}}}.$$

Hence we have the attraction-constant

$$=(0{\cdot}0172)^2\,\frac{\text{semi- major axis of earth's orbit}}{\text{day}^2\times\text{sun's mass}}$$

$$=\frac{(0{\cdot}0172)^2\times(19{,}900{,}000)^3}{(86{,}400)^2\times 320{,}000}\ \frac{\text{geogr. mile}^3}{\text{sec.}^2\times\text{earth's mass}}$$

$$=\frac{(0{\cdot}0172)^2\times(19{,}900{,}000)^3\times(7420)^3}{(86{,}400)^2\times 320{,}000\times\frac{4}{3}\times 3{\cdot}14\times(6{,}370{,}000)^3\times 6000}\ \frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}}$$

$$=\frac{615}{10^{13}}\ \frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}},$$

if we take the mean mass of 1 cubic metre of the earth as equal to 6000 kilogr. The force with which a mass of 1 kilogr. attracts an equal mass at a distance of 1 metre is then $\frac{615}{10^{13}}\ \frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}$. If we transform the value of the constant of attraction into the gravitation system, we obtain

$$\frac{615}{10^{13}}\ \frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}}=\frac{615}{10^{13}}\ \frac{\text{metre}^3}{\text{sec.}^2\times\frac{1}{9{\cdot}808}\ \frac{\text{sec.}^2\times\text{kilogr. (Paris)}}{\text{metre}}}$$

$$=\frac{615\times 9{\cdot}808}{10^{13}}\left(=\frac{604}{10^{12}}\right)\frac{\text{metre}^4}{\text{sec.}^4\times\text{kilogr. (Paris)}}\dagger.$$

7. Of systems with four fundamental units, that with fundamental units of length, time, mass, and force is not without interest. In order to obtain such a system we must retain two constants in the fundamental equations (2), (3), (6),

* *Theoria Motus Corporum Cœlestium*, p. 2.

† Wüllner (*Lehrbuch der Experimental-Physik*, vol. i. 1870, p. 145) puts the mass of a cubic metre of the earth =6, and hence finds for the attraction-constant the value $\frac{6128}{10^{11}}$, which value, if the unit of mass (=9·808 kilogr.) employed in the same work (vol. i. p. 59) is to be retained, is $\frac{1000}{9{\cdot}808}$ times too great, and must be altered to

$$\frac{9{\cdot}808}{1000}\times\frac{6128}{10^{11}}=\frac{601}{10^{12}}$$

(very nearly the same as given above). The unit of mass,

$$1\,\frac{\text{sec.}^2\times\text{kilogr. (Paris)}}{\text{metre}}=9{\cdot}808\ \text{kilogr.},$$

therefore attracts an equal mass at a distance of 1 metre with the force $\frac{601}{10^{12}}$ kilogr.

and (7). If we choose for this purpose the constant of equation (6) (the force-constant) and that of equation (7) (the attraction-constant), we obtain a system in which the dimensions of the chief units are given by equation (22). We have already found the value of the force-constant c_2 (example 1), namely

$$c_2 = \frac{1}{0\cdot009808} \frac{(\text{second})^2 \times \text{gramme (Paris)}}{\text{metre} \times \text{kilogramme}}$$

$$= \frac{1}{9\cdot808} \frac{(\text{second})^2 \times \text{kilogramme (Paris)}}{\text{metre} \times \text{kilogramme}}.$$

According to equation (22), the dimensions of the attraction-constant are $[c_1] = \left[\frac{l^2 k}{m^2}\right]$; we may find its new value as follows:—

$$c_1 = \frac{615}{10^{13}} \frac{(\text{metre})^3}{(\text{sec.}^2) \times \text{kilogr.}} = \frac{615}{10^{13}} \frac{(\text{metre})^2}{(\text{kilogr.})^2} \times \frac{\text{metre} \times \text{kilogr.}}{(\text{second})^2}$$

$$= \frac{615}{10^{13}} \frac{(\text{metre})^2}{(\text{kilogr.})^2} \times \frac{1}{9\cdot808} \text{ kilogr. (Paris) (equation 35)}$$

$$= \frac{615}{10^{13} \times 9\cdot808} = \frac{(\text{metre})^2 \times \text{kilogramme (Paris)}}{(\text{kilogramme})^2},$$

or

$$c_1 = \frac{627}{10^{14}} \frac{(\text{metre})^2 \times \text{kilogramme (Paris)}}{(\text{kilogramme})^2}.$$

The other mechanical equations of definition would remain unaltered. Thus we should have:—

for the work A, the relation $A = ks$, with the dimensions

$$[A] = [kl];$$

for the *vis viva*, $u = mh^2$,

$$[u] = \left[\frac{ml^2}{t^2}\right];$$

for the momentum, $q = mh$,

$$[q] = \left[\frac{ml}{t}\right];$$

and so on.

Such a system has this advantage for a beginner—that he is able to express either a force or a mass in units of weight, and so would escape the reduction (difficult to understand and often forgotten) of a force from units of weight into units of force of the Gauss-Weber system, or of a mass from units of weight into units of mass of the gravitation system. The following circumstance may, however, perhaps be considered inconvenient. The constant of force passes over into the equations which give the connexion between work and change of

vis viva, and between the so-called time-integral of force * $\int k\,dt$ and the change of momentum, as well as into certain other mechanical equations which are derived from equation (6).

The equations are as follows:—

$$A = \tfrac{1}{2} c_2 m (h^2 - h_0^2) = \tfrac{1}{2} c_2 (u - u_0),$$

$$\int k\,dt = c_2 m (h - h_0) = c_2 (q - q_0),$$

instead of

$$A = \tfrac{1}{2}(u - u_0),$$

$$\int k\,dt = q - q_0,$$

in the usual systems with three fundamental units. That the equations in question, as well as equation (6), are free from any special constant in the usual systems is, no doubt, to be formally mentioned; but the circumstance is not of practical importance. For physicists often, and engineers almost always, express both forces and masses in units of weight, and the reduction to absolute units necessary for the employment of the ordinary formulæ introduces a constant in a disguised form.

8. The different systems of electrical measurement arranged by Wilhelm Weber, which are connected with the magnetic system of Gauss, are of special interest.

We have the following equations between the chief conceptions of this group of phenomena:—

$$\mu^2 = ck\mathrm{L}^2, \quad \ldots \ldots \quad (36)$$

$$e^2 = ck\mathrm{L}^2, \quad \ldots \ldots \quad (37)$$

$$i = c\frac{e}{t}, \quad \ldots \ldots \quad (38)$$

$$i = c\frac{k\mathrm{L}^2}{l\mu}, \quad \ldots \ldots \quad (39)$$

$$i^2 = c\frac{k\mathrm{L}^2}{l^2}, \quad \ldots \ldots \quad (40)$$

$$\mathrm{E} = c\frac{kh}{i}, \quad \ldots \ldots \quad (41)$$

where μ denotes the quantity of free magnetism or strength of a magnetic pole, c the quantity of electricity, and i the current-strength; l is the length of an element of the current-path, L the distance between two elements of the current, two magnetic poles, or between a current-element and a magnetic pole; k is the attraction or repulsion between current-elements, magnetic poles, or electric masses. The magnetic pole is to

* Thomson and Tait, 'Theoretical Physics,' § 29.

be supposed to be in the line drawn from the centre point of the element acting on it, and at right angles to it; the two current-elements which act upon each other are to be regarded as parallel to each other and at right angles to the line joining their centres.

The complete formulæ are

$$ck = \frac{il\mu}{L^2}\sin\theta, \quad \ldots\ldots\ldots\ldots \quad (39\text{A})$$

$$ck = \frac{i^2l^2}{L^2}(\cos\epsilon - \tfrac{3}{2}\cos\theta\cos\theta_1; \quad \ldots \quad (40\text{A})$$

where ϵ, θ, and θ_1 are known angles. Equation (41) relates to the motion of a closed circuit in the neighbourhood of currents or of magnets; i is the strength of the induced current, k the electrodynamic action of the inducing currents or magnets on the induced conductor, estimated according to the direction of the motion; h is the velocity of motion, and E the induced electromotive force.

Since the units of length, time, velocity, and force are already fixed by a system of fundamental equations, we have six equations connecting the four new conceptions, μ, c, i, and E. In forming the corresponding fundamental equations we must therefore retain two constants. The electrostatic (or mechanical) system is obtained by retaining the constants of equations (36) and (40). In the electromagnetic system we retain the constants of equations (37) and (40). A very convenient system is obtained by retaining the constants of equations (38) and (40); this system may be called the Gauss-Weber system: it is completely worked out in Kohlrausch's *Leitfaden.* Lastly, we have the electrodynamic system with the constants of equations (37) and (39). The values of the constants are as follows in the Gauss-Weber system:—$c = \frac{1}{2}$ (equations 40 and 40 A), and $c = \frac{1}{e_0}$ (equation 38), where e_0 is the total quantity of electricity (positive and negative) passing per second with the unit current of this system. Weber and Kohlrausch have found

$$e_0 = 31 \times 10^{10}\,\frac{\text{millimetre}}{\text{second}}\text{*}. \quad \ldots\ldots \quad (42)$$

* Wiedemann, *Lehre vom Galvanismus und Elektromagnetismus,* vol. ii. div. 2. p. 457. Weber himself takes into account only one form of electricity, and puts therefore $e_0 = \frac{31 \times 10^{10}}{2}$ (more exactly $15{\cdot}537 \times 10^{10}$) $\frac{\text{millimetre}}{\text{second}}$ (*Abh. der königl. sächs. Ges. der Wissensch.* vol. iii. Also in Zöllner, *Principien einer elektrodynamischen Theorie,* pp. 93-131).

By the aid of IV. we easily find the following values of the constants in the other systems:—

Electrostatic system,

$$c=\frac{1}{e_0^2}\text{ (equation 36)},\quad c=\frac{e_0^2}{2}\text{ (equations 40 and 40 A)};$$

electromagnetic system,

$$c=\frac{1}{e_0^2}\text{ (equation 37)},\quad c=\frac{1}{2}\text{ (equations 40 and 40 A)};$$

electrodynamic system,

$$c=\frac{2}{e_0^2}\text{ (equation 37)},\quad c=\sqrt{2}\text{ (equations 39 and 39 A)}.$$

We obtain a formula of induction suited for practical use by replacing the force k in equation (41) by its value calculated from the formula (39 A) or (40 A).

Since the constant of equation (41) has the value unity in all four systems, it follows that the work performed by the current in unit time is equal to the product of electromotive force into current-strength. Moreover the equation corresponding to Ohm's law which determines resistance is of the same form in all three systems.

The relations of the chief units of the electromagnetic, electrodynamic, and electrostatic systems, together with their dimensions, are given in Prof. Wiedemann's *Lehre vom Galvanismus und Elektromagnetismus*, vol. ii. div. 2. pp. 467–471; they are also given for the electrostatic and electromagnetic systems in Maxwell's 'Treatise on Electricity and Magnetism,' vol. ii. pp. 239–245.

The occurrence of the constant e_0 in so many of the formulæ connected with electricity would seem to show that these formulæ could be derived from some natural law. Wilhelm Weber finds this in his *fundamental electrical law*,

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\frac{a^2}{16}\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

Here $\frac{1}{a}$ is the quantity of positive or negative electricity (measured electrostatically like e_1 and e_2) which traverses the circuit in unit time when the current-strength is equal to the electrodynamic unit current, *i. e.* is equal to $\frac{1}{\sqrt{2}}$ electromagnetic unit. Consequently we have $\frac{1}{a}=\frac{e_0}{2\sqrt{2}}$ or $\frac{a^2}{16}=\frac{1}{2e_0^2}$,

where e_0 is to be taken in accordance with equation (42). The fundamental law may then also be written

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\frac{1}{2e_0^2}\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\frac{d^2\mathrm{L}}{dt^2}\right\}\right]. \quad . \quad (43)$$

9. Systems with two fundamental units result by eliminating one constant from the fundamental equations for systems with three units. Various systems with two fundamental units may be formed by elimination of the constant of attraction. If in the value of this constant $\left(\frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2\times\text{kilogr.}}\right)$ we put

$$1 \text{ kilogr.} = \frac{615}{10^{13}}\frac{\text{metre}^3}{\text{sec.}^2} = \frac{615}{10^4}\cdot\frac{\text{millim.}^3}{\text{second}^2}, \quad . \quad (44)$$

this constant becomes unity, and we obtain a system with the two fundamental units of length and time. The unit of mass derived from the metre and the second is consequently

$$1\,\frac{\text{metre}^3}{\text{second}^2}=\frac{10^{13}}{615}\text{ kilogramme.}$$

The unit of force 1 kilogramme (Paris)

$$=9{\cdot}808\frac{\text{kilogr.}\times\text{metre}}{(\text{second})^2}=\frac{9{\cdot}808\times 615}{10^{13}}\frac{\text{metre}^4}{\text{sec.}^4};$$

consequently the new unit of force is

$$1\,\frac{\text{metre}^4}{\text{second}^4}=\frac{10^{13}}{9{\cdot}808\times 615}\text{ kilogramme (Paris).}$$

This system is employed for attraction-problems by Thomson and Tait ('Theoretical Physics,' §§ 459, 774)*. The density† of water $\left(=1000\ \frac{\text{kilogr.}}{\text{metre}^3}\right)$ becomes in this system $\frac{615}{10^{10}}\ \frac{1}{\text{sec.}^2}$ (thus independent of the unit of length). In order to compare this number with the number given by Thomson and Tait (§ 774) we must multiply it by $\frac{6}{5{\cdot}5}$, since Thomson and Tait put the mean specific gravity of the earth equal to 5·5. We

* Compare also H. Weber and Kohlrausch (Zöllner, *Principien einer elektrodynamischen Theorie*, p. 129); Maxwell, 'Treatise on Electricity and Magnetism,' vol. i. p. 4.

† Density is here taken to mean mass of unit volume of the substance (Maxwell, p. 5).

thus obtain for the density of water the number

$$\frac{6}{5{\cdot}5} \times \frac{615}{10^{10}} \frac{1}{\text{sec.}^2} = \frac{6{\cdot}7}{10^8} \frac{1}{\text{sec.}^2},$$

which agrees exactly with Thomson and Tait's number. It is to be observed that the dimensions of the electric and magnetic units in this system have exponents which are whole numbers. In the Gauss-Weber system the dimensions of $[\mu]$ and $[e]$ are the same as of $[m]$; for i we have the dimensions $\left[\frac{l^2}{t^2}\right]$, for $[E']$ $\left[\frac{l^3}{t^3}\right]$, and so on.

Example 8.—The Weber unit of current

$$=1\,\frac{\text{mm.}^{\frac{1}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}} = \frac{1}{10^{\frac{9}{2}}} \frac{\text{metre}^{\frac{1}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}}$$

$$= \frac{\sqrt{615}}{10^{\frac{9}{2}} \times 10^{\frac{13}{2}}} \left(= \frac{\sqrt{615}}{10^{11}}\right) \frac{\text{metre}^2}{\text{sec.}^2} \text{ (equation 44).}$$

If we wish to return to the ordinary system, we must observe equation (44). We may calculate as follows:—

$$\frac{\sqrt{615}}{10^{11}} \frac{\text{metre}^2}{\text{sec.}^2} \text{ (current-strength)} = \frac{\sqrt{615}}{10^{11}} \frac{\text{metre}^{\frac{3}{2}}}{\text{sec.}} \times \frac{\text{metre}^{\frac{1}{2}}}{\text{sec.}}$$

$$= \frac{\sqrt{615}}{10^{11}} \left(\frac{\text{metre}^3}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \frac{\text{metre}^{\frac{1}{2}}}{\text{sec.}} = \frac{\sqrt{615}}{10^{11}}$$

$$\times \frac{10^{\frac{13}{2}}}{\sqrt{615}} \frac{\text{kilogr.}^{\frac{1}{2}} \times \text{metre}^{\frac{1}{2}}}{\text{sec.}} \text{ (eq. 44)} = \frac{1}{10^{\frac{9}{2}}} \frac{\text{metre}^{\frac{1}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{second}}$$

(Weber unit of current).

The constant of attraction may also be eliminated by putting

$$1 \text{ second} = \sqrt{\frac{615}{10^{13}} \frac{\text{metre}^{\frac{3}{2}}}{\text{kilogr.}^{\frac{1}{2}}}} = \frac{\sqrt{615}}{10^5} \frac{\text{millimetre}^{\frac{3}{2}}}{\text{milligr.}^{\frac{1}{2}}},$$

so that the unit of time becomes a unit derived from the unit of length and the unit of mass,

$$1\,\frac{\text{metre}^{\frac{3}{2}}}{\text{kilogr.}^{\frac{1}{2}}} = \sqrt{\frac{10^{13}}{615}} \text{ seconds.}$$

The unit of force $1\,\frac{\text{metre} \times \text{kilogr.}}{\text{second}^2}$ becomes $\frac{10^{13}}{615} \frac{\text{kilogr.}^2}{\text{metre}^2}$,

from which the new unit of force

$$1\,\frac{\text{kilogr.}^2}{\text{metre}^2}=\frac{615}{10^{13}}\cdot\frac{\text{metre}\times\text{kilogr.}}{\text{sec.}^2}$$

$$=\frac{615}{9{\cdot}808\times10^{13}}\text{ kilogr. (Paris).}$$

The dimensions of the other principal units become

$$[a]=\left[\frac{m}{l^2}\right],\quad [h]=\left[\frac{m^{\frac{1}{2}}}{l^{\frac{1}{2}}}\right],\quad [e]=[\mu]=[m]$$

(according to the fundamental equations of the Gauss-Weber system),

$$[i]=\left[\frac{m}{l}\right],\quad \mathrm{E}=\left[\frac{m^{\frac{3}{2}}}{l^{\frac{3}{2}}}\right],$$

and so forth. A quantity of electricity is given in ordinary units: we find the electrostatic unit

$$1\,\frac{\text{millim.}^{\frac{3}{2}}\times\text{milligr.}^{\frac{1}{2}}}{\text{second}}=\frac{10^5}{\sqrt{615}}\text{ milligr.,}$$

or

$$1\text{ milligramme}=\frac{\sqrt{615}}{10^5}\cdot\frac{\text{millim.}^{\frac{3}{2}}\times\text{milligr.}^{\frac{1}{2}}}{\text{sec.}}\text{ Weber units;}$$

i. e. $\frac{10^5}{\sqrt{615}}$ or about 4000 milligr. ordinary matter has equal action at a distance as 1 Weber unit of electricity.

Instead of eliminating the constant of attraction, we may obtain a system with two fundamental units by eliminating the constant

$$e_0=31\times10^{10}\,\frac{\text{millim.}}{\text{sec.}}.$$

If, in doing this, we wish to make the unit of time a derived unit, we must put

$$1\text{ second }=31\times10^{10}\text{ millimetres.}$$

Time must therefore be expressed as a length, the new unit of time 1 millimetre $=\frac{1}{31\times10^{10}}$ sec.

The units of the four electric systems have the same dimensions and the same magnitude, with the exception of the units of the electrodynamic system. The dimensions of the most important units are as follows:—

$$[h]=[l^0m^0] \text{ (abstract number)},$$

$$[a]=\left[\frac{1}{l}\right],\quad [k]=\left[\frac{m}{l}\right],\quad [t]=[l],\quad [\mathrm{A}]=[m];$$

constant of attraction $[c_1]=\left[\frac{l}{m}\right]$, $[e]=[\mu]=[m^{\frac{1}{2}}l^{\frac{1}{2}}]$,

$$[i]=\left[\frac{m^{\frac{1}{2}}}{l^{\frac{1}{2}}}\right]=[\mathrm{E}].$$

We see that, by eliminating the constant (e_0), the distinction of the units by their dimensions is lost for the most part—a result which was to be expected, since e_0 has no independent dimensions, but is to be regarded as a velocity.

10. The systems with two fundamental units have yet another constant; if this be also eliminated, we have a system with only one fundamental unit. If we start with the system given above, with the fundamental units of length and time, we may eliminate the remaining constant c_0 by the relationship

$$1 \text{ sec.}=31\times10^{10} \text{ millimetres.} \quad . \quad . \quad . \quad . \quad (45)$$

We thus obtain a system in which the unit length is the only fundamental unit. The three electrical systems now combine into one. The dimensions of the units are as follows:—

$$[h]=[l^0] \text{ (abstract number)},\quad [t]=[l],\quad [a]=\left[\frac{1}{l}\right],$$

$$[m]=[l],\quad [k]=[l^0],\quad [\mathrm{A}]=[l],\quad [\epsilon]=[\mu]=[l],$$

$$[i]=[\mathrm{E}]=[l^0].$$

The distinction of units by their dimensions is now completely lost; velocity, force, current-strength, and electromotive force are abstract numbers; time, mass, work, quantity of electricity, and magnetic pole are to be regarded as lengths.

The unit-mass $1\,\frac{\text{millim.}^3}{\text{sec.}^2}$ $\left(=\frac{10^4}{615}\text{ kilogr.}\right)$ becomes equal to $\frac{1}{31^2\times10^{20}}$ millim. Consequently the unit of mass

$$1 \text{ millim.}=31^2\times10^{20}\frac{\text{millim.}^3}{\text{sec.}^2}=\frac{31^2\times10^{24}}{615}(=157\times10^{22})\text{kilog.},$$

or nearly one fourth of the earth's mass $(650\times10^{22}$ kilogr.).

Weber's formula may now be written

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\tfrac{1}{2}\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

Weber himself simplifies the formula thus:—

$$k=\frac{e_1e_2}{\mathrm{L}^2}\left[1-\left\{\left(\frac{d\mathrm{L}}{dt}\right)^2-2\mathrm{L}\frac{d^2\mathrm{L}}{dt^2}\right\}\right].$$

If we compare this formula with formula (43), we find that

it is not e_0, but $e_0\sqrt{2}$ which is eliminated by putting 1 sec. $= 31 \times 10^{10}\sqrt{2}$ millim. $= 44 \times 10^{10}$ millim. The new unit of time is $\frac{1}{44 \times 10^{10}}$ sec. The constant of attraction, too, is eliminated by Weber by introducing a unit of mass

$$\frac{2 \times 31^2 \times 10^{24}}{615} (= 314 \times 10^{22}) \text{ kilogr.}$$

(nearly half the earth's mass*). These last two formulæ appear to us equally simple; but the first of them, as we saw above, has the advantage of combining the three electrical systems into one. In returning to the ordinary systems, attention must be paid to equations (44) and (45).

Example 9.—The quantity of electricity 1000 millimetres is to be expressed in electrostatic units $\frac{\text{millim.}^{\frac{3}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}}$. We have 1000 millim. electricity

$$= 1000\,\frac{\text{millim.}^3}{\text{millim.}^2} = 31^2 \times 10^{20} \times 1000\,\frac{\text{millim.}^3}{\text{sec.}^2} \text{ (equation 45)}$$

$$= 31^2 \times 10^{23} \left(\frac{\text{millim.}^3}{\text{sec.}^2}\right)^{\frac{1}{2}} \times \frac{\text{millim.}^{\frac{3}{2}}}{\text{sec.}} = \frac{10^2}{\sqrt{625}} \times 31^2$$

$$\times 10^{23}\,\frac{\text{millim.}^{\frac{3}{2}} \times \text{kilogr.}^{\frac{1}{2}}}{\text{sec.}} \text{ (equation 44)}$$

$$= \frac{10^2 \times 31^2 \times 10^{23} \times 10^3}{\sqrt{615}} \left(= \frac{10^{28} \times 31^2}{\sqrt{615}}\right) \frac{\text{millim.}^{\frac{3}{2}} \times \text{mgr.}^{\frac{1}{2}}}{\text{sec.}}.$$

It is to be observed that a system with two or with only one fundamental unit is not completely determinate, unless its connexion to the ordinary systems with three fundamental units is given by relations such as equations (44) and (45). The systems with three fundamental units are therefore to be regarded as the fundamental systems of mechanics.†

11. The ordinary units of mechanics are not convenient nor

* Weber and Kohlrausch (Zöllner, *l. c.* p. 130).

† The above systems with less than three fundamental units may be regarded as the result of taking the unit of length, the unit of velocity, and the unit-of-attraction constant as fundamental units. If, for example, we take the millimetre, the velocity e_0, and the attraction-constant c_1 as fundamental units, we obtain, from $c_1 = 615 \times 10^{-4}\text{mm.}^3 \times \text{sec.}^{-2} \times \text{kilogr.}^{-1}$, 1 kilogr. $= 615 \times 10^{-4}\text{mm.}^3 \times \text{sec.}^{-2} \times c_1^{-1}$; and from $e_0 = 31 \times 10^{10}$ mm. $\times$ sec.$^{-1}$, 1 sec. $= 31 \times 10^{10} \times$ mm. $\times e_0^{-1}$; which expressions become identical with (44) and (45), if we omit c_1 and e_0 from the dimensions. The constants c_1 and e_0 should be left in the fundamental equations, so as not to disturb their homogeneity.

sufficient to give a quantitative representation of all the phenomena of physics; but we are obliged sometimes to introduce arbitrary units. Among these may be reckoned the unit angle, which is very useful in considering circular motion, for which purpose it replaces the unit of length. From the unit angle and the unit of time we derive the units of angular velocity and angular acceleration; since, further, moment of inertia corresponds to mass, and moment of rotation to force, we obtain a complete analogy with motion in a straight line, and equations analogous to the equations (2), (3), and (6). This analogy is lost by regarding the angle as an abstract number.

The conceptions derived from the phenomena of heat.—A quantity of heat w is proportional to the elevation of temperature T which it can cause in a mass m. We obtain thus the equation of definition,

$$w=cm\mathrm{T},$$

where c is a factor depending on the material nature of the body, which we will call the *capacity for heat*. The dimensions of the unit of heat are determined by the equation

$$[w]=[cm\mathrm{T}].$$

We have here three new units, of which we may choose two as fundamental units. Of the three possible combinations, we have determined upon one, and choose units for capacity $[c]$ and temperature $[\mathrm{T}]$ at pleasure, so that the unit of heat $[w]$ becomes a derived unit: it is the quantity of heat required to raise by one unit the temperature of the unit mass of a substance having unit capacity. The units of temperature in use are well known; for unit of capacity we take the capacity of water; the capacity of a body is therefore expressed by the same number as its so-called specific heat. As examples furnished by the science of heat we may take the following:—

The heat necessary to raise 10 kilogr. mercury (capacity $=0{\cdot}033$ capacity of water) from $0°$ C. to $100°$ C. $=0{\cdot}033\times 10\times 100(=33)$ kilogr. $\times$ Centigrade degree $\times$ capacity of water.

Coefficient of linear expansion of iron $=0{\cdot}000012\,\dfrac{1}{\text{degree C.}}$

$$=0{\cdot}000012\,\frac{1}{\frac{4}{5}\text{ degree R.}}=\tfrac{5}{4}\times 0{\cdot}000012\,\frac{1}{\text{degree R.}}$$

$$=\tfrac{5}{9}\times 0{\cdot}000012\,\frac{1}{\text{degree F.}}$$

Latent heat of ice $=79{\cdot}25$ degrees C. $\times$ capacity of water.

We obtain the work A which is equivalent to a quantity of heat w by multiplying this quantity by the mechanical equivalent

of heat Q,

$$A = Qw.$$

The dimensions of Q are

$$\frac{[A]}{[w]}.$$

We usually put

$$Q = 424 \frac{\text{metre} \times \text{kilogramme (force)}}{\text{kilogramme (mass)} \times \text{degree C.} \times \text{capacity of water}}$$

$$= 424 \times 3{\cdot}281 \times \tfrac{5}{9} (= 773) \frac{\text{English foot} \times \text{English pound (force)}}{\text{Engl. pound (mass)} \times \text{deg. F.} \times \text{capac. of water}}.$$

This value does not agree with either of the two ordinary mechanical systems, since both the unit of force and the unit of mass are here fundamental units. But if we introduce either

$$1 \text{ kilogramme (force)} = 9{\cdot}81 \times \frac{\text{metre} \times \text{kilogr. (mass)}}{(\text{second})^2},$$

or

$$1 \text{ kilogramme (mass)} = \frac{1}{9{\cdot}81} \frac{(\text{second})^2 \times \text{kilogr. (force)}}{\text{metre}},$$

we obtain in both cases

$$Q = 424 \times 9{\cdot}81 (= 4160) \frac{(\text{metre})^2}{\text{sec.}^2 \times \text{degree C.} \times \text{capacity of water}},$$

which value may be employed either with the Gauss-Weber system or with the gravitation system.

In stating physical magnitudes, we frequently see arbitrary units employed besides the absolute units: it is important in using such units to state their names completely, the names being often formed according to the theory of dimensions.

Example.—Electrolytic (chemical) unit of current

$$= \frac{\text{cubic centim. of electrolytic gas}}{\text{minute}} \text{ or } \frac{\text{milligramme copper}}{\text{minute}}.$$

The rules for the reduction to new units are then always applicable, as the above examples in heat show, and render the calculation rapid and certain.

Helsingfors, January 1881.

Postscript.—After the above was in type the author became acquainted with Dr. Herwig's work, *Physikalische Begriffe und absolute Maasse* (Leipzig, 1880). The units of the science of heat are stated somewhat differently by Herwig: the equivalent Q is first eliminated, so that the units of quantity of heat and of work become identical; secondly, the capacity of water is taken as a conventional unit, and its name is omitted from the dimensions.

XI. Moseley's *Theory of Steady Flow. By* Major Allan Cunningham, *R.E., Honorary Fellow of King's College, London**.

A NEW *Theory* of the Steady Flow of a Liquid was proposed by the late Canon H. Moseley, in an important essay published in this Magazine in 1871–72†. The paper was valuable as an attempt to form a rational theory of the steady motion of a real fluid, and to deduce results of great scientific and practical interest from it. Formulæ were arrived at for the *Velocity* at any point in a Pipe flowing full, and for the *Discharge* both of a Pipe flowing full and of an Open Channel, and for some other quantities of less importance.

In the investigation it was assumed that:—

I. *Pipes.*—In long uniform pipes at uniform slope, flowing full,—

1. The motion is a steady motion in lines parallel to the axis of the pipe.
2. The surfaces of equal velocity are similar, and similarly situate, to the enclosing margin.
3. The measure of tangential resistance to the fluid at the enclosing margin is $(\lambda_1+\mu_1 v^2)$.
4. The measure of tangential resistance to fluid particles flowing past each other is $(\lambda+\mu\,.\,(\delta v)^2)$.
5. The whole of the kinetic energy lost by any particle of the fluid under subheads 3 and 4 is probably converted into heat energy, or, at any rate, lost as kinetic energy.

II. *Open Channels.*—In long uniform open channels at uniform slope,—

6. The discharge is one half of the discharge of a pipe flowing full whose lower half is the open channel in question, and upper half is similar to the lower (reversed).

The evidence as to the reality of the assumptions Nos. 1, 2, 5 is not stated: they are apparently only working hypotheses. No. 3 is accepted from Poncelet's experiments on the friction of a fluid on a solid. No. 4 is merely an assumption that the expression for fluid-friction is of the same functional form as that between a fluid and a solid. No. 6 is accepted from Darcy's experiments on pipes and open channels.

It must be remarked, in passing, that there is really no evidence as to the existence of steady motion in parallel lines even in pipes flowing full, and that in the case of open chan-

* Communicated by the Author.

† Vol. xlii. 1871, pp. 184 and 349; vol. xliv. 1872, p. 30.

nels, at any rate, all modern experiment points* to the conclusion that the motion is *essentially unsteady* and that the *stream-lines interlace freely in all directions.* Next, hypothesis No. 2 is contrary to the evidence of Bazin's experiments. In Bazin's work two diagrams are given† of cross sections of two rectangular iron pipes (flowing full) laid with two sides horizontal, with velocity-measurements made at many points on several horizontal and several vertical lines thereof figured thereon: now the contours of loci of points of equal velocity traced thereon are all rounded (like ellipses); so that hypothesis No. 2 disagrees with nature.

From these hypotheses Nos. 1 to 5, the primary result is an expression for the *velocity* at any point in a pipe flowing full; and thence follows, by a simple integration, an expression for the *discharge* through the same. By help of No. 6 (which is a result of pure experiment) this last result is extended so as to give a formula for *discharge* in open channels.

It will be seen that the results for pipes (flowing full) are strictly dependent on the hypotheses Nos. 1 and 2 of steady motion in parallel lines, and of the surfaces of equal velocity being similar and similarly situate to the enclosing margin; whilst the formula for discharge in open channels involves no assumptions as to the actual motion therein, but depends only on the correctness of the theory of the motion in pipes flowing full together with the connecting link No. 6 (derived from experiment).

From the uncertainty of the assumptions, it is evident that the only test of the correctness of the theory is the comparison of numerical results therefrom with observation.

The extensive *small-scale* experimental results ‡ of MM. Darcy and Bazin on pipes and open channels not exceeding 2 metres in width nor $\frac{1}{2}$ metre in depth were applied (in the original essay) in numerical verification of the formulæ quoted —viz. for *Velocity* at any part of a circular pipe flowing full, and for *Discharge* both in pipes flowing full and in open channels. The result was *apparently satisfactory*; that is to say, there was certainly a remarkable approximation on the whole (with occasional very large discrepancies) between the experimental and theoretical results. A sound theory of flow of

* See 'Roorkee Hydraulic Experiments,' vol. i. chap. vi., for a summary of the evidence.

† *Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts*, pl. xviii.

‡ *Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux*, by H. Darcy. Paris, 1857.

Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts, by H. Darcy and H. Bazin. Paris, 1865.

water would be of such great scientific interest, and its ultimate result—a good formula for Discharge—would be of such great practical use in engineering, that it is well worth while to thoroughly test this new theory: it will be shown that the approximate agreement above noticed is quite illusory.

The primary result of the investigation is, as stated, a formula for the velocity v at any point at a distance r from the centre of a pipe of radius R flowing full, the central velocity (in the same cross section) being v_0, viz.

$$v = v_0 \,.\, \epsilon^{-\gamma r \div \mathrm{R}}.$$

Upon different hypotheses, and by a quite different investigation, M. Darcy proposed* the formula

$$v = v_0 \,.\, \left\{ 1 - m\left(\frac{r}{\mathrm{R}}\right)^{\frac{3}{2}} \right\},$$

and applied the same (his own) experimental results in verification.

It is quite clear that these two expressions, differing so greatly *in form*, cannot both be correct rational formulæ; and yet they both give numerical results agreeing sufficiently with Darcy's experimental results to have satisfied their proposers. But the fact is, that the numerical test relied on from Darcy's experiments is (though this seems to have escaped attention) a very poor one. Darcy's velocity-measurements were made at only five points in a vertical line through the centre of each pipe, viz. at the centre and at points symmetrically above and below the centre at $\frac{1}{3}$ and $\frac{2}{3}$ of the radius from the centre; thus embracing only the middle $\frac{2}{3}$ of the diameter in question, within which the change of velocity is very small, and the *velocity-curve* (or locus of the equation) is therefore very flat. Thus almost any very flat curve would agree tolerably well with the observations in the middle $\frac{2}{3}$ of the diameter, especially when the comparison is made (as in the present instance) between ordinates measured in so large a unit as a metre, as the difference (in metres) would then only be small decimals. The dissimilarity of the two curves is in this case very striking, their convexities being actually *turned opposite ways*. Thus Moseley's curve is concave downstream with a cusp at the middle, whilst Darcy's is convex downstream with an apse at the middle. On plotting Darcy's *observations* they will be found to give curves generally very flat, convex downstream with an apse at the middle; so that Moseley's velocity-formula does not agree with nature.

All the rest of the investigation depends on this primary

* *Recherches expérimentales* &c., by H. Darcy, p. 128.

velocity-formula, and therefore fails as a *rational theory* along with it. It might, however, happen that the other results might prove to be good empirical formulæ. The only others admitting of test are the formulæ for Discharge, which are given in several forms, as suited for pipes of various sections (circular, rectangular, &c.) flowing full and for open channels. A large number of the discharge-measurements of MM. Darcy and Bazin in circular pipes flowing full and in small rectangular and trapezoidal open channels, are compared in the original essay with the results computed from the formulæ; and here, again, there is a remarkable approximation in the results. This turns out, however, to be no criterion of approximation on the large scale, as will appear below.

Some extensive *large-scale* experiments on flow of water in large canals of various widths up to 200′ and depths up to 11′, and various discharges up to 7000 *cubic feet per second*, made under the author's superintendence near Roorkee in Northern India, in 1874–79, and recently published*, afford a test for the formula for discharge in open channels on the large scale. The formula given is

$$\left.\begin{array}{r}\text{Discharge } \textit{in cubic}\\ \textit{metres per second}\end{array}\right\} D'=\tfrac{1}{2}\frac{B'}{R'}\cdot\{1-(1+2R')\cdot\epsilon^{-2R'}\}\cdot v'_0,$$

where

B' = wetted border, R' = hydraulic mean depth, both *in metres*;
v'_0 = central surface-velocity *in metres per second*;
D' = discharge *in cubic metres per second.*

The formula is a very laborious one for computation; but its failure on the large scale can be readily shown, without complete computation, as follows:—It is obvious that the quantity $(1+2R')\cdot\epsilon^{-2R'}$ is always a + quantity, rapidly decreasing as R' increases: hence the above expression gives a result always *less than* $\frac{1}{2}\frac{B'}{R'}\cdot v'_0$ *cubic metres per second*, and therefore (changing to British measures) the

$$\left.\begin{array}{r}\text{Discharge, } \textit{in cubic}\\ \textit{feet per second}\end{array}\right\} D \text{ is always } <\frac{(3\cdot281)^3}{2}\times\frac{B}{R}\times\frac{v_0}{3\cdot281},$$

$$\text{or } <5\cdot382\frac{B}{R}\cdot v_0,$$

where B, R, v_0 are here measured *in feet.*

* Roorkee Hydraulic Experiments, 1881, 3 vols. (published at Thomason C. E. College, Roorkee, N.W. P., India, 1881).

A few selected instances from the Roorkee experiments*, with widely differing data (as to width, depth, velocity, and discharge), are given in the table below.

Site.	Data.			Value of $5{\cdot}382\frac{B}{R}v_0$.	Discharge-measurement D.
	B.	R.	v_0.		
Soláni left aqueduct	100·2	6·75	4·05	324	2328
Soláni right aqueduct	105·8	7·96	4·52	323	3429
Soláni embankment, main site...	190·8	9·34	4·66	512	7169
Fifteenth mile, new site	182·0	9·49	4·91	507	7187
Belra site	196·4	9·02	3·67	430	5611
Jaolí site	200·2	7·82	3·43	473	4631
Kamhera site	69·5	4·84	3·40	263	961

It will be seen that the computed values of $5{\cdot}382\frac{B}{R}v_0$, which *should be all greater than* the discharge-measurements (D), are actually only from about $\frac{1}{4}$ to $\frac{1}{14}$ of the latter; so that the formula is evidently useless for application to large bodies of water.

The Roorkee experiments also furnish† important (negative) evidence on the vexed question of the supposed convexity of the water-surface in an open channel. Canon Moseley states (in the same essay‡) that, as a result of theory,—

> "As the pressure is everywhere less where the velocity is greater, it is evident that there will be a tendency in the liquid on the surface to flow from the sides of the channel towards the centre, and that thus the velocity of the surface-water at the centre will be diminished, and the water heaped up, drowning, as it were, the point of greatest velocity in the section."

Now two of the results above indicated—viz. the constant transference of surface-water § from the edges towards the centre and the depression‖ of the maximum velocity below the surface—have been amply verified experimentally. But as to the "heaping-up" of the surface-water at the centre, or, in other words, as to the surface-convexity (the existence of which seems to be accepted by writers on hydraulics), the author has not been able to find any certain experimental evidence. Some special experiments were accordingly made by him to test the point, at a site *extremely favourable* for the trial, in a canal of about 170′ surface-width and 11′ depth, with a central sur-

* Roorkee Hydraulic Experiments, vol. i. p. 297.
† *Op. cit.* vol. i. chap. viii. *passim.*
‡ Philosophical Magazine, [4] vol. xliv. p. 44.
§ Roorkee Hydraulic Experiments, vol. i. chap. xvii. arts. 14–14 *b*.
‖ *Op. cit.* vol. i. chap. xii. *passim.*

face-velocity of about 4·5 *feet per second*. The experiment (fully detailed in the Roorkee work*) was a very delicate one, on account of the incessant slight oscillations of the water-surface; but every possible care was taken. The result of twelve trials on one day and twenty-four on another, was that the mean water-surface (*i. e.* average of the oscillations) at the centre and edges was (on a calm day) *most probably level.*

XII. *On the Influence of Time on the Change in the Resistance of the Carbon Disk of* Edison's *Tasimeter*. *By* T. C. Mendenhall, *Columbus, O.*†

ABOUT five years ago Edison announced the discovery of the remarkable property possessed by carbon when prepared in a special manner, in virtue of which its electrical resistance was greatly lessened by subjecting it to an increase of pressure. Among the numerous interesting applications of this discovery which were quickly made, none was more promising or more interesting than the tasimeter devised by Edison himself. The extreme sensitiveness of the carbon to the slightest changes in pressure gave rise to the hope that the instrument would far exceed in delicacy those previously in use for the detection of minute quantities of heat.

Mr. Edison was a member of the Draper Eclipse Expedition in the summer of 1878, and used his tasimeter during the total eclipse of July 29 in that year, attempting to measure the heat emitted by the sun's corona. His report to the director, Dr. Henry Draper, was published in the Proceedings of the American Association for the Advancement of Science for the same year. This report shows that the attempt was by no means as successful as could have been desired, the principal obstacle being apparently the difficulty in the adjustment of the tasimeter so that the galvanometer-needle would remain at zero, and to secure its return to that point after it had been deflected. In fact, the zero adjustment was only made by the use of a peculiar shunt of variable resistance ingeniously contrived by Mr. Edison for the purpose.

The writer is not aware of any other systematic attempt to secure quantitative results through its use; and, as far as known, the instrument has been generally regarded as peculiarly inconstant and unreliable in its indications.

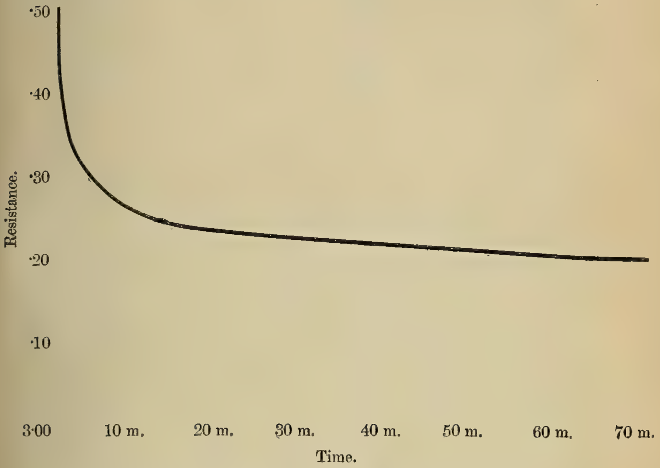
Having in his possession a tasimeter constructed after the

* *Op. cit.* vol. i. chap. viii. arts. 4, 4 *a*.

† From Silliman's American Journal of Science for July 1882, having been read, by invitation, at the April meeting of the National Academy.

model of that described in the report referred to above, the writer undertook a short time ago to investigate the quantitative relation between pressure and resistance for the carbon disk which belonged to it. In a series of preliminary experiments, the use of the toothed wheel and screw, by means of which the pressure is communicated to the disk, was found to be extremely objectionable on account of the impossibility of exactly reproducing a given pressure. This portion of the instrument was therefore entirely removed, and an arrangement made by means of which any definite pressure might be quickly brought to bear upon the disk or removed from it. A slender brass rod was placed in a vertical position upon the centre of the upper contact piece, the upper end of which rested lightly in a small conical cavity made on the underside of the scale-pan of a balance. The weight was suspended above by a fine thread passing over a pulley; so that by raising or lowering it, the pressure was applied or removed as was desired. The carbon disk was made one of the branches of a Wheatstone's bridge, as described by Mr. Edison. In lowering the weight, care was taken to make the movement slow enough to avoid any shock to the disk. When the apparatus stood with the weight lifted, the adjustment of the galvanometer to the zero was made without any difficulty, the resistance of the disk appearing to be quite constant. When the pressure was applied, however, the adjustment became very troublesome; and after a few trials it was discovered that *time* was a very important element in the problem. The addition of a pressure of fifty grams reduced the resistance to nearly one fourth of what it was in its normal condition *instantly*; but it was found that the minimum was not reached at once. The resistance continued to fall during the first two or three minutes with considerable rapidity, and after that more slowly. A series of experiments was accordingly undertaken for the investigation of this phenomenon. After a number of trials, the bridge was adjusted so that when the key was closed simultaneously with the application of the pressure the needle of the galvanometer would remain momentarily at zero; for the instantaneous effect of this pressure seemed to be quite constant. In a few seconds, however, the needle began to move, showing that the resistance was diminishing. With this constantly decreasing resistance it was, of course, difficult to obtain balances which were very accurate; but generally one could be obtained within a minute after the application of the pressure, and another a minute or two later, and so on. The operation was repeated many times, and a number of points for the curve shown

below were obtained, which, though necessarily somewhat scattering, were so situated as to render its general form almost certain. In almost every instance, immediately after the removal of the pressure the normal resistance was again measured; and it was found that while time was necessary for the resistance to reach a minimum after the application of the pressure, the disk seemed to recover its maximum normal resistance instantly upon its removal.



Curve showing the relation between Resistance and Time.

After the construction of the curve showing the relation between time and resistance, and on the supposition that it correctly represents that relation, it was easy to know what the adjustment of the bridge should be at the end of any given time; and thus the difficulty of that adjustment disappeared.

When tested in this way, the curve was found to be correct within the errors of experiment. The following table exhibits the resistances after various times, the *instantaneous* resistance being called 100. The resistance before the addition of the pressure of 50 grams was 11·67 ohms, which immediately fell to 3·52 ohms upon the application of the weight.

Time, in minutes.	Resistance.	Time, in minutes.	Resistance.
0	100	15	92·9
1	96·6	20	92·5
2	95·4	25	92·3
3	94·9	30	92·1
4	94·5	35	92·0
5	94·2	40	91·8
6	93·9	50	91·5
7	93·7	60	91·2
8	93·6	70	90·9
9	93·4	80	90·8
10	93·3	90	90·7
12	93·1		

It will be seen that the resistance falls a little more than 3 per cent. in one minute, about 5 per cent. in three minutes, and about 10 per cent. in one and a half hours; and it seems tolerably certain that even then a minimum is not reached. In two or three instances the time of continuous pressure was prolonged to twenty-four hours, the resistance at the end being slightly lower than at any previous reading. Finally, the apparatus was left with the weight applied for one week. No measurements were made during that time; but at the end the resistance was found to be decidedly lower than it was at the end of two hours after the application of the pressure; and it is especially to be noticed that, on the removal of the pressure, the normal resistance of a week before was instantly recovered. In this case the pressure applied was 100 grams. The resistance before the application of the pressure was 11·08 ohms. Upon applying the pressure, it immediately fell to 2·34 ohms. In two hours this had been reduced to 2·10 ohms; and at the end of a week it was 1·93 ohm. Thus in two hours it was reduced by about 10 per cent.; and after one week it was again about 10 per cent. lower.

It appears, therefore, that the element of time plays an important part in the phenomena exhibited by the carbon disk; and it seems highly probable that this has been one of the principal causes, if not the chief cause, contributing to the inconstancy and unreliability of the indications of the tasimeter. The experiments made thus far indicate a fair degree of constancy in its results when this factor is considered. The writer hopes to be able to make further examination concerning the extent to which all the conditions necessary to its use may be controlled.

The resistance of carbon under pressure has been made the

subject of investigations recently by Mr. Herbert Tomlinson and Professor Silvanus Thompson. The conclusion reached by both is that the diminution of resistance is really due to the contact between the electrodes; and it appears that Professor W. F. Barrett has arrived at a similar conclusion, as a result of experiments made upon a "button of compressed lampblack." Without knowing any thing about the nature of these experiments, the writer desires to record his belief that this theory does not entirely account for the facts stated above. Besides, it seems a little difficult to understand how so small a pressure as fifty grams, added to an already existing pressure of about the same amount, can increase the area of contact between a flat plate and a flat disk nearly four times, to say nothing of the "recovery" which takes place so promptly upon the removal of the pressure.

XIII. *Crystallographic Notes.* By W. J. Lewis, *M.A.**

[Plate III.]

PSEUDOBROOKITE.—A pupil of mine, whilst making a list of the apatites in the Brooke collection preparatory to their registration, called my attention to a specimen of asparagus-stone from Jumilla, Murcia, on which were some minute black crystals of apparently rhombic symmetry. They were clearly not hematite, which is frequently found in thin laminæ in the matrix from this locality. The measurements obtained show it to agree well with the mineral discovered by Dr. Koch (Groth's *Zeitschrift f. Krys.* iii. p. 306), which he has called pseudobrookite.

The crystals are very small, and consist of simple prisms with the makrodiagonal pinakoid, strongly striated parallel to their intersections, and terminated by minute bright dome-planes (figs. 1 and 2). They have a considerable tendency to more or less parallel growth; and the opposite planes of the prism are not, as a rule, accurately parallel. They are brittle, and seem to have no good cleavage.

The quantity so far obtained from the specimens in the Cambridge collection is very small (not more than one grain), and it has been impossible to get a chemical analysis made. A preliminary examination by Dr. Hugo Müller confirms, to a certain extent, the belief that it is identical with Koch's pseudobrookite. The following table gives the angles observed and calculated, as also the corresponding angles given by Dr. Koch:—

* Communicated by the Crystallological Society, having been read June 3, 1882.

	Lewis. Calculated.	Lewis. Observed means.	Koch. Calculated.	Koch. Observed.
am	25° 49′	*25° 49′ (mean of 5 best) 25 45½ (mean of 20)	25° 51′	26° 31′
al	44 3⅓	......	44 6	44 6
ae	69 1½	69 2 (mean of 5)	68 50	68 56
$ee_{\prime}$	41 57	*41 57 (mean of 6)	42 20	41 19
me	71 12	71 7½ (mean of 6)		
$me_{\prime}$	108 48	108 42		

The development requires us, in my opinion, to take m as (1 1 0). The plane e might conveniently have the indices (1 0 3); and the elements would then be:—

System rhombic.

$$D=(0\,1\,0,\,0\,1\,1)=29°\,5'{\cdot}5;\quad E=(0\,0\,1,\,1\,0\,1)=48°\,59'{\cdot}6;$$
$$F=(1\,0\,0,\,1\,1\,0)=25°\,49'.$$

or $$a:b:c=1:2{\cdot}067:1{\cdot}150.$$

The mineral is specially interesting, as, from Dr. Koch's analysis, it seems to be a compound of ferric oxide and titanic acid, and to be therefore a dimorphous form of ilmenite. Its crystal-elements do not approach those of Brookite sufficiently near to justify us in considering it isomorphous with the latter mineral. It offers, therefore, a fresh instance of the peculiar connexion which exists between oxide of iron and oxide of titanium.

Ludlamite.—In the Brooke collection is a specimen with a label in Heuland's handwriting, "Phosphate of Iron on a fossil, Stösgen near Linz on the Rhine, *new.*" The phosphate of iron is in minute pale green translucent crystals, having the characteristic three-faced-wedge habit of ludlamite on the free terminations. The best crystals of Cornish ludlamite, as described by Prof. Maskelyne, give angles which vary greatly, owing to more or less parallel growth. The crystals from Stösgen, although half a dozen different crystals have been tried, have such imperfect faces that no measurements have been yet obtained which render a comparison with the ludlamite from Cornwall possible. They seem to have a good cleavage, which, however, manifests the irregular growth of the minute crystals by the indefiniteness of the reflexion obtained from the cleavage-face. They are deposited on the sides of a cavity in the midst of a small mass of greenish-grey matrix, which consists of bundles of coralloid structure containing apparently a quantity of the same substance. I hope during the

course of the long vacation to be able to settle definitely the crystallography of the mineral.

Idocrase.—The Brooke collection contains a small crystal of idocrase, apparently from Zermatt, which has minute planes adjoining the base which do not seem to have been hitherto noticed. They are striated, and the measurements obtained are not good. Using p and $p_{/}$ to denote two of the planes in a zone with c, and $p_{//}$ one of the others, the measurements obtained were $cp=6^\circ\ 3'$ (best), $cp_{/}=6^\circ\ 19'$, and $pp_{//}=9^\circ\ 26'$. The form $(1\,1\,7)$ is that which agrees best with these measurements; the angles required by it being $(0\,0\,1,\ 1\,1\,7)=6^\circ\,10\tfrac{1}{5}'$, and $(1\,1\,7, 1\,\bar{1}\,7)=8^\circ\ 42\tfrac{1}{2}'$.

Zoisite.—In the Cambridge collection is a small specimen of zoisite in small bright green crystals imbedded in calcite. One of them had terminal planes; and by careful extraction from the calcite, I obtained a crystal showing four terminal pyramidal faces. The faces were rough, and deeply striated parallel to the edge lying in the brachydiagonal plane, so that the measurements obtained were not good. They agree, however, sufficiently well with those given by Brögger (Groth's *Zeitschrift*, iii. p. 471); and they have led me to the conclusion that Brooke must have been mistaken in giving the angle $wk=56^\circ\ 30'$, an angle which has been assumed in all the mineralogical works to give the elements of the crystal. DesCloizeaux observed a poor dome-plane, of which I seemed to have doubtful indications, and has given a table of angles calculated from Brooke's data. The positions of the faces are shown in the stereographic projection (fig. 3). The angles given by M. DesCloizeaux are compared in the following table with those of Brögger and with those observed and calculated by me.

	Calculated.	Means of observed angles.	Angles adopted by Brögger.	Angles calculated by Dx.
bs	*58° 8′	58° $9\tfrac{1}{2}$′	*58° 17′	*58° 8′
sk	14 36	14 36		
$ss_{/}$	63 44	63 35	63 26	63 44
bw	*73 9	73 $10\tfrac{1}{2}$ 73 19 (Miller)	73 9	72 $28\tfrac{1}{2}$
sw	56 42	56 $52\tfrac{1}{2}$	56 $32\tfrac{1}{2}$	55 $13\tfrac{1}{2}$
$ww_{//}$	66 36	66 14	66 55	69 33
aw	62 $12\tfrac{1}{2}$	62 54	......	61 1
ww_3	55 35	55 5	55 57	57 58
$s_{/}w$	75 56	75 35 (Miller)	75 43	75 23
wk	57 $54\tfrac{1}{3}$	.....	......	*56 30
$ll_{/}$	58 19	60° near (Dx.)	......	61 3

The plane a is doubtful. The planes k were only measured on one side of the crystal; and a, possibly a result of repetitions of the opposite k planes, was found on the other side. The difference in the angles $w\,w_{,,}$ and $w\,w_3$ given by Brooke's data and those adopted by me suffice, I think, to justify my belief in an erroneous impression as to the prism-plane which gave the reading 56° 30′. The angles $b\,s$ and $b\,w$ are unmistakable, and are those used in determining the elements; the remaining angles found by Brögger and myself agree as nearly as can be expected with those calculated.

Quartz.—In the Cambridge collection are two crystals of quartz, each of which has a well-developed plane whose indices were determined by the late Prof. Miller to be $(50, \bar{19}, \bar{19})$. Professor Miller seems never to have published this result; nor has any record of the measurement which led to it been found amongst his papers. I therefore remeasured the crystals, and obtained an angle which agrees almost exactly with that required by the indices $(50, \bar{19}, \bar{19})$. The larger crystal is a broken prism about 40 millim. long by 8 millim. across. The face y on this crystal is about 1·5 millim. long by about ·75 millim. broad, and is smooth and bright. Near the edge $[by]$ it is slightly rounded. As shown in the diagram (fig. 4), it has to the left a large rough x plane and a long somewhat narrow s plane. The r plane above y is developed so as to all but blot out the other terminal planes, and is traversed by a few horizontal lines, due to repetition of r and some plane in the zone $[r\,y]$. At the extreme top the plane r is more strongly striated, and is penetrated in a perfectly arbitrary way by small crystals of quartz which are ill developed. In the zone $[z\,r_{,}]$ are three minute planes, angular measurement of which places them in the position s, l, u. These planes, however, do not succeed one another in this order, but form re-entrant angles, u being that adjacent to $r_{,}$, l next, and s last, and adjacent therefore to $b_{,,}$. The planes u and l are strongly striated parallel to the edge $[b_{,,}\,s]$. The planes observed on this crystal are $b\,(2\,1\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,\bar{1})$, $y\,(50\,\bar{19}\,\bar{19})$, $\phi\,(8\,\bar{13}\,8)$, $s = \alpha\,(4\,\bar{2}\,1)$, $x\,(4\,\bar{2}\,1)$, $u\,(6, 17, \bar{12})$ or $(5, 14, \bar{10})$, $l\,(3\,10\,\bar{6})$.

The smaller crystal is a slender prism about 13 millim. long. The plane y on it is not so well developed as in the former crystal, and it is considerably more rounded near the edge $[by]$. The image, however, given by it is quite distinct. The plane s to the right of y is very largely developed; and x and r appear as very narrow planes below it. The crystal is a combination of $b\,(2\,\bar{1}\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,\bar{1})$, $y\,(50\,\bar{19}\,\bar{19})$, $x\,(4\,\bar{1}\,\bar{2})$, $v\,(16\,\bar{5}\,\bar{8})$, and a plane near ϕ.

The planes observed on these two crystals are given by the stereographic projection (fig. 5).

Mr. Thomas Davies, of the British Museum, lent me a crystal which showed a very prominent plane below the s plane, as also some narrow ones, looking somewhat like striations, between them. The distribution of the faces on this crystal is shown by the stereographic projection (fig. 6). It is a combination of $b\,(2\,\bar{1}\,\bar{1})$, $r\,(1\,0\,0)$, $z\,(2\,2\,1)$, $s=\alpha(4\,1\,\bar{2})$, $g=\alpha(3\,2\,\bar{4})$, $t\,(5\,3\,\bar{6})$, $s'\,(7\,2\,\bar{4})$, $x\,(4\,\bar{1}\,\bar{2})$, $\chi\,(\overline{15}\,8\,8)$.

Direct observations of t and s' in the zone $[b_{//}\,s]$ were too little reliable to be of any service. These planes were therefore determined by observations of the angles they make with b and $z_{//}$. The plane s' is somewhat doubtful. The faces w and n are very rounded, and no reliable measurement could be made.

The planes $(50\,\overline{19}\,\overline{19})$, $(3\,2\,\bar{4})$, $(5\,3\,\bar{6})$, $(7\,2\,\bar{4})$ are not given by M. DesCloizeaux; and this is possibly the first time that their existence has been recorded. The following table gives the observed and calculated angles for these planes:—

	Calculated.	Observed.
	° ′	° ′
ry . . .	30 25$\frac{1}{6}$	30 23$\frac{1}{2}$ on the larger crystal. 30 20$\frac{1}{2}$ on the smaller crystal.
$z\chi$ . . .	36 15	36 1
$r_{/}s$ . . .	28 54	28 53
$r_{/}l\,(3\,10\,\bar{6})$	33 47	33 57 not good.
$r_{/}u\,(6\,17\,\overline{12})$	38 9	37 52 " "
$r_{/}u\,(5\,14\,\overline{10})$	38 29	"
$b_{//}g$ . . .	10 19	10 16
$b_{//}t$ . . .	13 41$\frac{1}{2}$	......
$b_{//}s_{/}$ . . .	34 33	35 0 near.
bg . . .	52 39	52 43
$bt\,(5\,3\,\bar{6})$.	50 44	50 29
$bs'\,(7\,2\,\bar{4})$.	39 10$\frac{1}{2}$	39 10$\frac{1}{2}$
$z_{//}t$ . . .	30 42	30 28$\frac{1}{2}$
$z_{//}s'$ . . .	27 44	28 0

XIV. *On the Dimensions of a Unit of Magnetism in the Electrostatic System of Measures.* *By* R. Clausius*.

THREE articles in the June number of the Philosophical Magazine, respecting my determination of the dimensions of the electrostatic unit of magnetism, induce me to give some further explanations on the point of view from which I started in that determination.

One of the greatest advances of physical science was Ampère's establishment of the connexion between magnetism and current electricity, and his pointing out that, in respect of its action, a small magnet can be replaced by a small closed electric current, of which the quantity and intensity stand in the following relation to the strength of the magnet:—the product of the intensity and the surface round which the current flows is equal to the moment of the magnet. In consequence of this principle, magnetism need no longer be regarded as a peculiar and separately existing agent, but to the word magnetism a notion can be attached the definition of which is to be drawn from electrodynamics.

But then it is evident that this definition must be such as to correspond with Ampère's proposition universally, and independently of the system of measures employed, and not such that a current and a magnet which are equivalent when one system is employed become of different values on the employment of another. On this condition I have based my determination of the static unit of magnetism.

Now, with respect to Maxwell's treatment of the matter, he has in several passages of his work quoted Ampère's proposition as a correct one, without anywhere adding any limiting remark to the effect that the proposition is to be regarded as valid only in the electrodynamic system, and not in the electrostatic. His units, however, he has determined in such wise that Ampère's proposition is satisfied only in the electrodynamic system, while in the electrostatic the quantities which according to Ampère should be equal are represented by expressions which have different dimensions, and hence their values are changed in quite different ways by an alteration of the fundamental units.

How Maxwell arrived at his formula for the electrostatic unit of magnetism, which deviates from Ampère's proposition, it is impossible to say with certainty, as no explanation about it is given in his work. Nevertheless, as I have already said in my previous paper, it can be inferred from the context that

* Translated from the German MS. communicated by the Author.

the way in which he has brought into the calculation the force acting between an electric current and a magnetic pole determined the derivation of his formula.

If we suppose given an electric current of intensity i passing along a straight line of infinite length, the force exerted by the current upon a magnetic pole m at the distance L of the straight line will be represented by $2imL^{-1}$, provided that i and m be measured in electrodynamic measure. If, then, each of the three quantities i, m, and L is a unit of the kind of quantity in question, the expression takes the value 2 and represents 2 units of force. Hence, introducing the formula of the unit of mechanical force $[MLT^{-2}]$, we can write

$$2[imL^{-1}]=2[MLT^{-2}],$$

from which follows

$$[im]=[ML^2T^{-2}],$$

and, if for $[i]$ we substitute $[eT^{-1}]$,

$$[em]=[MLT^{-1}].$$

This is the equation of Maxwell's, cited in my preceding paper, in which for $[e]$ he has inserted the expression of the electrostatic unit of electricity in order to get the electrostatic unit of magnetism.

It is, however, to be remarked that in forming this equation the formula of the unit of mechanical force is put for an electrodynamic force, and thereby the equation obtains the character of an electrodynamic equation, into which we must not directly put electrostatic units.

In opposition to this, Mr. Everett says that my equation derived from Ampère's proposition stands, in respect of the property of being electrodynamic, on a par with Maxwell's. But this I must controvert. Ampère's proposition enunciates that the forces exerted by a magnet and by the corresponding electric current are *equal to one another*, but does not represent those forces by any formula. Maxwell's equation, on the contrary, rests, according to the above derivation, upon the introduction of a definite formula for the electrodynamic force, namely the formula of the unit of mechanical force ; and it is this circumstance that makes it an electrodynamic equation, in the sense that it is not suitable for application to electrostatic units, as the determination of the latter rests upon the representation of a quite different force (the electrostatic force exerted by two quantities of electricity upon each other) by the formula of the unit of mechanical force.

In Mr. J. J. Thomson's article another objection appears, in

the words "Mr. W. D. Niven has pointed out to me that the value given by Clausius for the dimensions of a magnetic pole does not make the magnetic force between two such poles of the dimensions of a force, which ought clearly to be the case." I cannot understand this objection. In the *electrodynamic* system of measures the force between two magnetic poles $[m_d]$ is represented by the formula of mechanical force. Now, if in the *electrostatic* system also the force between two magnetic poles $[m_s]$ is to be represented by a formula of the same dimensions, then must $[m_s]$ have the same dimensions as $[m_d]$, which is no more the case with Maxwell than with me, and also cannot be the case; while if instead of $[m_s]$ the quantity is put which I have denoted by v. d. $[m_s]$ (the value of the electrostatic unit of magnetism reduced to electrodynamic measure), the expression of the force between two magnetic poles assumes again the dimensions of the mechanical-force formula. Mr. Niven might have made the same objection against the electrodynamic unit of electricity as he has made against the electrostatic unit of magnetism.

J. J. Thomson raises also an objection of his own. He says that, in determining magnetic force the *magnetic permeability* μ of the medium in which the current is placed must also be taken into account; and then he adds :—"Thus the force between two magnetic poles depends on the medium in which they are placed; but, according to Maxwell, the magnetic force between a current and a magnetic pole does not." In this sentence a distinction is made between the force of a magnetic pole and the magnetic force of a current, which is quite foreign to Ampère's theory. According to that theory a small closed current can completely replace a small magnet with respect to the forces exerted by it upon other magnets or closed currents, so that it is no longer necessary, in order to explain the magnetic actions of a body, to assume the presence in it of a special agent to be designated by the name of magnetism, but instead of this we can suppose that the molecules are encircled by electric currents, and that these exert the actions in question. It is of course contradicting this to say that the magnetic forces proceeding from an electric current act according to other laws than those which govern the forces proceeding from a magnet.

For all these reasons I must continue to maintain that, if physicists accept Ampère's theory (which is certainly done by most physicists, and, as I believe, was done also by Maxwell), they must, to be consistent, accept also the formula determined by me for the electrostatic unit of magnetism, instead of the formula set up by Maxwell.

XV. *On Double Refraction, produced by Electrical Influence, in Glass and Bisulphide of Carbon.* *By* H. Brongersma*.

[Plate IV. fig. 5 *a–h*.]

I.

THE phenomena first observed by Kerr†, and described in his papers entitled "A new Relation between Electricity and Light," I have submitted to a reexamination. The importance of the subject, and the failure of the attempts of various physicists to repeat Kerr's experiments, so far as these relate to solid bodies, have induced me to undertake this investigation.

The willing readiness of the Directors of the "Teyler's Stichting" at Harlem to place an apartment and the necessary instruments at my disposal has made it possible for me to accomplish this labour, which, as I hope, has resulted in proving that the doubt of the correctness of Kerr's results was not well founded.

If I mistake not, Gordon was the first who repeated Kerr's experiments, and, indeed, in spite of all his carefulness, with a negative result‡. In a work published subsequently§, he returns to the subject. After describing the phenomena which he had once observed when the glass was perforated by the spark, he says:—"A fresh glass plate was at once drilled, in hopes of repeating the experiment in the lecture next day; but, *owing to sparks springing round*, we did not succeed in perforating the glass, and therefore saw only the faint return of light described by Dr. Kerr." The words which I have italicized make it in some degree doubtful if the phenomenon observed by Gordon must not be attributed to accidental causes. I have found that a piece of ordinary plate glass is rendered doubly refracting only by the electric spark passing in proximity to it, or when a heated wire is brought near the plate.

Mackenzie‖ also did not succeed in obtaining Kerr's phenomenon. He thought it was produced by heat only.

An investigation by Röntgen¶ likewise gave negative results. He also believes that some accidental influences were at work in those experiments.

* Translated from Wiedemann's *Annalen*, 1882, no. 6, pp. 222–233.
† Phil. Mag. [4] vol. l. p. 337 (1875).
‡ Phil. Mag. [5] vol. ii. p. 203 (1876).
§ A Physical Treatise on Electricity and Magnetism, by J. E. H. Gordon, vol. ii. p. 247.
‖ Wiedemann's *Annalen*, ii. p. 356 (1877).
¶ Ibid. x. p. 77 (1880).

In a copious memoir*, Quincke briefly touches on this part of Kerr's investigation. He says:—"In fact, plate glass and bisulphide of carbon show, according to Dr. Kerr's observations, opposite electric double refractions; in my experiments I found this confirmed for flint glass and bisulphide of carbon." After the many negative results attained by others, it is be regretted that Quincke has not described in detail his method of experiment with glass, as in the other parts of his investigation.

Lastly, Grunmach† did not succeed in observing the phenomenon.

After this I think I ought to communicate my own results, and the more so since, according to them, Kerr himself has not seen these phenomena in their whole extent.

In a piece of common plate glass 14 centim. high, 6 centim. broad, and about 1 centim. thick, two holes about 3 millim. in diameter were drilled coaxially, parallel with the largest lateral surface, so that their ends remained 5 millim. distant from one another. Into each of these openings a small quantity of mercury was introduced, and then thin-drawn-out glass tubes were fixed therein with a mixture of shellac and wax, through which fine copper wires 15 millim. in length were carried till they reached into the mercury. They were then thickly coated to their ends, together with a portion of the glass plate, with the mixture above mentioned. Moreover the plate was varnished, except at the two places which bounded the field of view.

Much care was necessary in doing this, in order to ensure the possibility of obtaining a satisfactory difference of potential. The investigation had to be interrupted several times, partly because the insulation was not sufficient, and partly because the plate was perforated by electric sparks.

The glass plate was set up midway between two nicols 15 and 11 millim. in diameter respectively, so that the incidence of the light-rays was perpendicular, and the space between the wires or electrodes occupied the centre of the field. The light of an albo-carbon lamp (which was placed in a sort of Duboscq lantern), before passing through the polarizing nicol, fell upon a lens, so that the real image of the round aperture of the lantern coincided with the centre of the plate. Afterwards, with the aid of a second lens, mounted in front of the analyzing nicol, a magnified image was observed by the eye; and it was soon found that by this alteration the method was improved. The electric charge was obtained by means of a Holtz machine, with the conductors of which the copper wires were connected.

* Wied. *Ann.* x. p. 537 (1880). † Ibid. xiv. p. 110 (1881).

In the first experiment the planes of polarization of the crossed nicols made an angle of 45° with the horizon (first position). Without a glass plate there was, in the middle of the field, a dark spot; the borders of the field were not completely dark. When the glass plate was inserted, there appeared, principally in consequence of the drilling, phenomena of double refraction as in fig. 5 *c*.

Soon after the Holtz machine was brought into action the field changed: first the dark portion between the electrodes divided into two parts; and two dark tails made their appearance, which joined the electrodes to one another above and below, leaving a strongly illuminated space of elliptical form between them. Gradually these tails receded from each other at one of the electrodes, the ellipse approaching more to a parabola. Fig. 5 *a* shows the phenomenon as it appeared when the difference of potential was the greatest possible. After about three minutes' powerful working of the machine, the field underwent no further perceptible change; the maximum action had therefore been attained. After a sudden discharge the phenomenon vanished, at first rapidly, and then more slowly; and within three minutes every thing was again as before the experiment.

Having placed before the analyzing nicol a plate of calc spar polished perpendicularly to the axis, I saw, on repeating the experiment, the black cross in the plate change into a hyperbola; so that the light must have been elliptically polarized.

When, instead of the calc-spar plate, I took a vertically compressed glass plate, and slowly increased the pressure, the tails of fig. 5 *a* approached one another, at last they assumed the above-mentioned elliptic shape, and with still stronger pressure the phenomenon disappeared.

If the calc-spar plate was now again placed before the analyzing nicol without any pressure taking place upon the compensating glass plate, with increasing difference of potential the black cross, as remarked above, was converted into a hyperbola. By gentle vertical pressure of the compensating glass plate the hyperbola was again converted into the black cross.

If the polarization-planes of the nicols were directed horizontally and vertically (second position), the phenomenon appeared as in fig. 5 *b*.

The phenomena of double refraction with the first position were seen by Dr. Kerr incompletely; and he did not succeed in observing those which accompany the second position, perhaps because his field of view was too small to take in the whole. By placing the glass lens in front of the analyzing

prism and mounting the plate movable horizontally and vertically, by which different parts could be successively brought into the field, I succeeded in improving his method. It is, however, very possible that the kind of glass used by Kerr was not without influence upon the result. According to my experiments a piece of very hard English glass, of the same thickness (18 millim.) as that employed by Kerr, becomes strongly doubly refracting in consequence of the drilling. With it the described phenomena were only faintly shown.

Of other glass plates, some, which in consequence of the drilling had become much more strongly doubly refracting than those above mentioned, yielded perceptible but much less distinct results.

The above is besides in complete accordance with the result derived by Kerr from his experiments—namely, that glass under the action of electrical influence behaves like glass which is compressed in the direction of the lines of force.

The difficulties in this investigation are not inconsiderable. For it is necessary to make the difference of potential very great; and then many a glass plate is perforated by sparks. Besides this, it is not easy to insulate sufficiently the conducting connexions. To this must be added that many glass plates become so strongly doubly refracting in consequence of the drilling as to be useless for the investigation. This was the case chiefly with plates whose apertures were drilled with a metallic instead of a diamond drill, presumably in consequence of the great pressure attending the employment of the former. The influence of the nature of the glass I have not yet been able to observe.

Kerr saw in these phenomena a confirmation of Faraday's theory respecting dielectrics. That great physicist already regarded it as probable that under the influence of electricity an isotropic body passes into the anisotropic state, so that it behaves like a doubly refracting crystal. He did not, however, succeed in confirming this by experiment. Still, in my opinion, it is not sufficiently proved that these phenomena cannot be of a secondary order. The motions of the molecules, on their arrangement in a limited portion of the plate, may have for their consequence a development of heat; and this, again, may be the cause of the observed double refraction, as Werner Siemens has already experimentally demonstrated by the heating of the insulating medium of a condenser accompanying the charge and discharge *. I hope that a continued investigation will soon enable me to enunciate a definite view.

* Berl. *Monatsber.* 1864, p. 614.

II.

In his subsequent memoirs* Kerr treats of double refraction of liquids produced by electrical influence. Röntgen has repeated Kerr's experiments†. He made use of a larger nicol than Kerr employed, and by so doing gained the important advantage of being able to have a better view over the whole. The results of the two investigations agreed in the main. Only, with the horizontal and vertical position of the polarization-planes of the nicol, Kerr observed no or only an irregular light-phenomenon; while according to Röntgen's observations the phenomenon with this position of the nicol was complementary to that accompanying the first position. Röntgen, however, appears not to have seen this phenomenon in its entirety, as will be shown subsequently.

In my experiments a square glass jar 5 centim. broad and 9 centim. high, filled with bisulphide of carbon, was employed. In the centres of two parallel sides, apertures of about 3 centim. diameter were made, which were again closed by glass plates 0·2 centim. thick. The jar was set up midway between the nicols, upon an ebonite stand, so that the light-rays fell perpendicularly onto the thin glass plates. Apertures were also made in the centres of the two other sides, to admit the electrodes. The latter were tightly screwed on copper wires, which, inclosed in thin glass tubes, were fixed in the apertures with sealing-wax. Each of these copper wires was connected with one of the conductors of the Holtz machine.

In order to avoid particles of dust, the bisulphide of carbon had to be repeatedly filtered from a bottle into the experiment-vessel and *vice versâ*. Finally the vessel was filled with bisulphide of carbon from a third bottle.

In the first experiment one electrode was a brass disk of 12·7 millim. diameter and 7·8 millim. thickness, the other a sphere of 8·5 millim. diameter. The axis of the disk was horizontal; and its prolongation passed through the centre of the sphere. The polarization-planes of the nicols were constantly perpendicular to one another, and made an angle of 45° with the horizon, and consequently also with the direction of the lines of force in the centre of the field of view. As soon as the machine worked, the phenomenon represented in fig. 5 *e*, as nearly as possible, was observed. The middle of the field was brightly illuminated, and most brightly in the immediate vicinity of the electrodes, which is not given in the figure.

* Phil. Mag. [4] l. p. 446 (1875); [5] viii. pp. 85, 229 (1879); [5] ix. p. 157 (1880).

† Wied. *Ann.* x. p. 80 (1880).

Two black tails issue from the sphere, at points the radii belonging to which make an angle of 90°, which is bisected by the axis of the disk. Two other black tails issue from the disk, whose directions at the beginning likewise form an angle of 45° with the horizon. A glass plate placed before the analyzing nicol, upon the centre of which the rays fell perpendicularly, had, with an extremely gentle horizontal pressure, the following influence upon the phenomenon:—With slowly increasing difference of potential, first a small black bow, nearly in the form of a semicircle, with its centre in the point of the surface of the spherical electrode where the prolongation of the disk-axis cut that surface, was observed. Further, this bow divided into two branches connecting two points of the sphere with two points of the disk. These branches receded constantly further from one another till at length fig. 5 *e* again came into view. If the glass plate was pressed somewhat more forcibly, the two first-mentioned phases of the phenomenon, and with still stronger pressure the first only, namely the black bow, were observed. If the pressure was still further enhanced, the entire field remained uniformly illuminated, even when the potential-difference was the greatest possible. If, with a great potential-difference, the horizontal pressure upon the glass plate is slowly increased, the same phenomena follow in reverse order, from fig. 5 *e* to the above-described black bow, which is seen to become slowly smaller till at last it vanishes.

If on the repetition of the experiment without the compressed glass plate the potential-difference is slowly increased, the same transitions can be remarked, as it appeared to me, as were observed when a gently compressed plate was employed; only the first transitions were slight and less distinctly marked, so that already with a proportionally slight difference of potential the phenomenon is seen as in fig. 5 *e*, at first faint, but gradually coming out sharp and clear. If the origin of the black tails at the sphere be joined to its centre, the angle which the lines thereby formed make with the horizon, on the potential-difference diminishing and without a horizontally compressed glass plate, or with the potential-difference constant and with increasing horizontal pressure upon the glass plate, becomes smaller; but in no case, not even with the greatest possible potential-difference, does this angle become greater than 45°.

If the glass plate was exposed to a vertical pressure, with increasing pressure the tails issuing from the sphere and those issuing from the disk removed further from one another, so that the illuminated intervals at the sides of the electrodes

became smaller (as in fig. 5 *e*). At last these tails were completely squeezed against the electrodes; and with still stronger pressure they disappeared. With a greater difference of potential they emerge again, to again vanish in consequence of stronger pressure. Both with horizontal and with vertical pressure of the glass plate, a slight difference of potential with gentle pressure has the same effect as a great potential-difference with stronger pressure. The latter agrees with what was found by Röntgen.

When I substituted for the compressed glass plate a plate of calc spar cut perpendicularly to the axis, I convinced myself that here also the light was elliptically polarized. Before the machine worked, the coloured rings with the black cross appeared. When the machine commenced working, the latter changed into a hyperbola; the ends of the two branches of the hyperbola receded further from each other as the potential-difference increased.

If the polarization-planes of the nicols were brought into a horizontal and a vertical position, the phenomenon of fig. 5 *f* showed itself already at a slight potential-difference; at a greater difference it became sharp and distinct. With this position Kerr did not obtain any evident results, probably because in his experiments the field of view was too small for him to see the whole of the phenomenon. But how it was that Röntgen* saw only the black horizontal line that issues from the sphere is so much the more inexplicable, as he with different electrodes, and with vessels of different widths, always found the same results. I have repeated this experiment with electrodes corresponding exactly in dimensions with those employed by Röntgen. Yet this did not alter the phenomenon, any more than an alteration of the distance of the electrodes from 2·7 to 5 millim. In frequent repetitions of this experiment it was always seen by me unchanged; and even the results obtained with two spherical electrodes agree with this, as will be subsequently shown.

With horizontal and vertical pressure upon the glass plate while it again occupied the previously mentioned position, the phenomenon was unchanged if the glass was placed so that the black cross was midway between the electrodes.

When the direction of the pressure upon the glass plate made an angle of 45° with the horizon, the figure was unsymmetrical. This is also the case when the nicols are gradually moved out of the second into the first position. The figures then form a transition between fig. 5 *e* and fig. 5 *f*. The latter also accords with Röntgen's results.

* See fig 5 of his paper.

In a further experiment the electrodes were brass balls of 8·5 millim. diameter. With the nicols in the first position fig. 5*g* appeared; in the second, fig. 5*h*. Horizontal and vertical pressure upon the glass plate had the same effect as in the first experiment. I obtained completely accordant phenomena also with spherical electrodes of larger dimensions. If one electrode was a sphere of 14 millim. and the other a sphere of 8·5 millim. diameter, this made just as little difference.

At first I thought that a movement of the electricity from one of the electrodes to the other would have a great influence upon the phenomenon. This induced me to make the following experiments.

Two cylindrical electrodes were placed coaxially. One of their opposed surfaces was furnished with fine points. Fig. 5*d* shows the phenomenon which was observed with the nicols in the first position. That which appeared with the second position was very similar to fig. 5*f*.

In the next experiment two thermometers whose spherical bulbs had an external diameter of 13 millim. were employed as electrodes, their mercury being conductively connected with the Holtz machine: thereby neither a dark nor a spark-discharge could take place through the liquid; so that the potential-difference could be raised very high. The striking-distance of the machine was now as great as when the condensers were not connected to the electrodes. The phenomena observed, both with the first and with the second position of the nicols, agreed with those obtained with spherical electrodes (figs. 5*g* and 5*h*). Only with the first position of the nicols was the field illuminated between the electrodes, while the black tails at the sides of the electrodes were much less distinctly visible. With the second position such a difference was not to be perceived. On repeating this experiment with spherical copper electrodes completely inclosed in glass tubes, I obtained the same results.

Further, it is noteworthy that the liquid, which in the experiments with uncovered electrodes was constantly in motion, now remained at rest. Only during a few seconds, as the machine was beginning to work, were very delicate undulations to be seen when the polarization-planes of the nicols formed an angle of about 90° with one another, so that the field of view was feebly illuminated previously.

That with Kerr's and Röntgen's method of experiment a dark discharge takes place through the liquid was ascertained by both physicists; and they deem this fact not unimportant for the explanation of the phenomena. From the experiments

just mentioned, however, it is evident that such a discharge is not requisite, any more than a violent motion of the liquid.

That view would also be untenable for this reason, because we have here to do with elliptically polarized, and not with depolarized light.

If it is further shown that explanations which rest upon the hypothesis that electricity may produce these phenomena indirectly, are not confirmed by an experimental investigation, it becomes more and more probable that we have to do here with a hitherto unknown action of electricity on the light-undulations; and then Kerr's phenomena, as Röntgen justly says, acquire an extraordinarily fundamental significance.

I hope soon to touch this subject again.

Harlem, March 1882.

XVI. *Notices respecting New Books.*

(i) *Mathematical Papers by* WILLIAM KINGDON CLIFFORD. *Edited by* ROBERT TUCKER, *with an Introduction by* H. J. STEPHEN SMITH. London: Macmillan and Co. 1882. (8vo, pp. lxx, 658.)

(ii) *Mathematical Fragments, being Facsimiles of his unfinished Papers relating to the Theory of Graphs, by the late* W. K. CLIFFORD. London: Macmillan and Co. (Fol., pp. 22.)

TO those who remember the late W. K. Clifford's appearance as a rising mathematician of singularly brilliant promise, one likely to extend the limits of knowledge in any of the various fields of Mathematics to which he might be led to apply his powers, his short career of little more than a decade would seem a mere dream, but for the substantial evidence of his energy and productiveness contained in the thick volume of more than six hundred octavo pages (one third consisting of matter in close type) which we have here to notice. And it is to be borne in mind that the publication of this volume has been preceded by the collection of two volumes of Lectures and Essays produced subsequently to Clifford's removal from Cambridge to London in 1871. The former publication (of more popular matter, but handled invariably with characteristic scientific precision and freshness), together with the collection which forms the subject of the present notice, may interest (and they are deeply interesting) from either of two points of view,—one of which has occupied the pen of Prof. H. J. S. Smith in a masterly Introduction; and the other will be more particularly here dwelt on—the study of the growth of a singularly gifted genius as it derived fresh aliment from extended acquaintance with the work and speculations of kindred minds, and visibly waxed stronger and stronger in its own powers by their exercise on problems of ever increasing height and subtlety.

The absence of any kind of arrangement of these Papers with reference either to subject or date is much to be regretted—"tantum series juncturaque pollet." For the absence of chronological order an apology is offered in the Preface; and some amends are made by a chronological Table therein drawn up, which rather serves to show that the apology might have been rendered needless, as far as any difficulty in assigning, with at least approximate accuracy, their dates to the few doubtful papers. At all events, the chronological order of the greater part is clear of doubt; and had this order been followed, the effect, on a reader able to follow intelligently and appreciatively the matter of the Papers, would have been a sense of harmonious development of an almost unique genius among the English mathematicians of his generation.

Thus, in the earliest papers (contributed to the three leading mathematical serials of this country, and covering the period from 1863 to the completion of his undergraduate career, with the next year or two) reference is made to the higher geometrical text-books only, which occupy the attention of the candidate for a good place on the Tripos: they contain no strikingly new results, but rather reproduce, with extensions and novelties of algorithm, results already known. These papers are of an exclusively geometrical character.

During the three or four years intervening between taking his degree and his removal to London (1871) his mathematical communications were made to the Cambridge University Philosophical, and London Mathematical Societies, or to Section A of the annual British Association Meeting—the one exception being a geometrical paper commenced in the 'Messenger,' October 1869 (erroneously assigned to 1870 in the chronological list), in which he urges the greater cultivation of the methods of Synthetic, or Organic, Geometry; and, drolly enough, makes a reference (no doubt second-hand) to St. Thomas Aquinas, of all authorities on a geometrical question! Among these papers occur two purely analytical:—the "Proof that every Rational Equation has a root" (which attracted much attention at the time), and the closely connected (though separated by some 140 pages in the reprint) "Case of Evaporation in the Order of a Resultant." In both these papers the subjects are treated with a masterly conciseness. The "Lecture Notes," drawn up, as we are informed, for tutorial lectures at Trinity College, Cambridge, in 1870, show a gradually extended course of reading. They commence with the emphatic words "Geometry is a physical science" (probably adopted from Mill, 'Logic,' ch. xxiv.); and in them Riemann is for the first time quoted—a mathematician whose short but brilliant career of Professor Extraordinary at Göttingen had terminated in 1866, after a few years of struggle against the disease which was sapping his life, as, a few years later, it was to lay Clifford in his grave. There is pervading these "Notes" a strong flavour of Hankel's *Vorlesungen über die complexen Zahlen* (1867), which probably accounts for the references to Riemann, Gauss, Argand (otherwise a name almost unknown to mathema-

ticians of the present generation, but whom Hankel characterizes as the true founder of the method of representing a complex geometrically), Cauchy, and Grassmann.

The years 1871, 1872 produced only five short Mathematical Papers, the most considerable being an extension to tridimensional space of a plane theorem of the late Mr. Cotterill's. In the Editorial note to the first of these (p. 234) "nodal *conic*" should be "nodal *cone*." In 1873 appear evidences—from his translation of Riemann's Essay "on the Hypotheses which lie at the basis of Geometry," and the "Preliminary Sketch of Biquaternions"—of Clifford's thoughts having been directed to speculation on hyper-dimensional space, a subject reverted to in a succession of papers down to the close of his career—the last being the unfinished "Classification of Loci" (Phil. Trans. 1878). On the whole of this subject Prof. H. J. S. Smith's masterly analysis will be the reader's best guide. To the same year belongs the first of his two contributions to the Phil. Trans., "On Mr. Spottiswoode's Contact Problems." The year 1874, though one of great activity in other scientific directions, produced no published mathematical papers; the abstracts of his communications to the British-Association Meeting showing that Clifford took his part in the Peaucellier "revival" of the period, and in following up the idea of bringing Chemical Equations under a general formula.

The published mathematical papers of the next three years were contributions to the Mathematical Society's 'Proceedings,' those which attracted most attention being:—(i.) "On the Transformation of Elliptic Functions" (1875),—suggested by a paper of Dr. Lüroth's in the *Mathematische Annalen*, Bd. I.,—followed by "Notes" thereon (1876), in which Darboux's priority both in matter and method on certain points is acknowledged and the geometric proof of the transformation-formulæ is restated and completed; (ii.) "On the Canonical Form and Dissection of a Riemann's Surface"—a most characteristic example of Clifford's powers in its kind. But these three years produced many papers left incomplete by the author and now printed as left. Some, which are mere fragments, were apparently the commencements of papers to be offered to the Societies or Mathematical Journals; others are more probably compilations to form the bases of courses of Lectures planned, but destined never to be carried out. A singular and doubly melancholy contribution to the volume is the late Miss Watson's series of notes of the course of Lectures on Quaternions which, as a student of University College, London, she attended. This was probably the first instance of public instruction in Quaternions offered in England. In Edinburgh it has been for some years a recognized subject in the Mathematical Curriculum, having been introduced by the late Prof. Kelland and by Prof. Tait—the most skilled adept in the method among English mathematicians, and whose elementary Treatise has been the source whence most of the younger mathematicians who have

paid attention to the method have derived their first knowledge of its principles. It is one of the losses which University College, London, has most to lament in Clifford's death, that with him has apparently died the effort to establish the elements of Quaternions in its Mathematical programme. Strangely enough, in France, too, the most flourishing School of Quaternions is not to be found in Paris, but in the provincial Faculty of Bordeaux. Apart from the interest of their authorship, it might be a question whether Clifford would have wished these Lecture Notes to have been published among his own papers. Whatever there is novel in the treatment is probably incorporated in his 'Dynamic;' and a cursory examination of a page or two indicates the necessity of a revision of the Notes by some one conversant with the subject. Thus at p. 502, "a point of no *velocity*" is an error for "no *acceleration*," and the whole paragraph is a corollary to one in the following page; wherein, again, a reference to "equation (2)" should be a reference to another equation not numbered, just above. Such errata as $\dot{O}$ for $\ddot{O}$ are either typographical or consequences of the hurry of note-taking.

Of much more general interest will be the unfinished "Algebraic Introduction to Elliptic Functions," in which, though Prof. Smith finds that it "contains no new results and perhaps no original methods of investigation," the most recent contributions to the subject by Rosenhain, Königsberger, Schröter, Göpel, Cayley, and Smith had been incorporated. This "Introduction" was probably drawn up with immediate reference to his intended course of lectures in University College. The third most considerable paper in the Appendix, "On Power Coordinates in general," should certainly have been preceded by the Notes on the Theory of Powers which follow it. The remarks (p. 555) with which the Editor has introduced these 'Notes' are very unfortunate: if "they are, in places, *apparently* inaccurate" and "it is not *easy* to see how the equations in (1) and (4) are got, nor how the other equation in (1) contains a linear relation between the powers of a point with respect to a &c.,"—of course *cela dépend*; but if Mr. Tucker had referred his difficulties in this case, as he did in others, to any of the many mathematicians who would gladly have cleared them up, it would have been pointed out to him that the results are perfectly accurate to a *factor près* (as it might have been expected any such work of Clifford's would be), and that the last equation in (1) has the significance assigned to it. In the last equation but one of (4) as printed there is certainly an erratum of the sign $(-)$ for $(\times)$; but this would probably not be found in the MS.

Of great interest is the collection of Problems and Solutions from the 'Educational Times;' and it appears that there is still a goodly number of Questions proposed by Clifford remaining unanswered, the solutions of some of which would perhaps be found, in substance at least, among the Papers and Fragments now for the first time published.

It would be vain to attempt to convey in a cursory notice any adequate idea of the contents of the thin folio of lithographed facsimiles of the fragments on "Graphs," a subject which formed one of Clifford's communications to the 'Proceedings' of the British Association in 1875, "On the Graphical Representation of Invariants."

To those who are acquainted with Clifford's mathematical papers, it is needless to remark how invigorating, disciplinary, and suggestive the study of them is—and that they are for the most part by no means easy of reading, but, on the contrary, require a good deal of hard work on the part of the reader to cover with shorter steps the long strides with which he gets over his ground. The reader of this collection is deeply beholden to Prof. H. J. S. Smith for the lucid and helpful analysis of the contents with which he has introduced them, and to Professor Cayley for his elucidatory notes to many of the posthumous papers; also to Mr. Spottiswoode, P.R.S., as well for similar assistance as, we believe, for his liberality in undertaking the cost of lithographing the Fragments on Graphs. The labour which Mr. Tucker has contributed as editor it is superfluous to point out; and the circumstances which induced him to undertake so heavy and responsible a charge are explained in the Prefatory Letter. It is the best return we can make an editor for such labour in our behalf, to point out whatever imperfections and errata come under observation, for amendment in the Second Edition, which the interest likely to be taken in this collection may be expected to call for ere long.

[By the Editors' kindness I have been permitted to see the above Review in "proof," and to append to it the few remarks which follow. "Triangular" symmetry, I need hardly say, is Clifford's own title to his paper, though in line 9 he writes, "I call . . . . . . rectangular symmetry." In the Bibliography I state that the *lower* date of publication is given: the Reviewer is no doubt aware that alterations are frequently made by authors in their papers in the interval which elapses between composition and publication: I think, but I am open to correction, that the paper (viii.) was not published in its entirety until 1870. The Reviewer implies, in writing of my remarks on p. 555, that I made the arrangement without consulting any one else; but I can assure him that these two papers were submitted to the, I believe, careful consideration of one of my four referees; and it was on his advice that I took the course I adopted and wrote the "very unfortunate" remarks. I am now able to say that my difficulties have been cleared up, or nearly so, by a young mathematician who is working on some of Clifford's lines. For a second edition, if such should be called for, the Reviewer's remarks, and any further ones he may be willing to send me, will be most acceptable.—R. TUCKER.]

A Treatise on the Theory of Determinants, with graduated Sets of Exercises for use in Colleges and Schools. By Thomas Muir, *M.A.* Macmillan: London, 1882. (Pp. vi+240.)

The subject of Determinants is every day coming more and more to the front. As evidence of this we may instance the recent works of Mr. Scott and Mr. W. Thomson, and the chapters in Messrs. Burnside and Panton's 'Theory of Equations.' Students are no longer shut up to the advanced works of the masters in the science nor restricted to the small morsels dealt out in two or three of our algebraical textbooks.

In France M. Dostor has recently brought out his *Eléments de la Théorie des Déterminants avec application à l'Algèbre, la Trigonométrie et la Géométrie analytique dans le plan et dans l'espace*; and in this country Mr. Muir, who has done so much good original work, now puts forth the excellent text-book under notice. Without going into any lengthened detail, we may say that the Author does not touch upon the Geometrical applications of Determinants; perhaps he considers that these have been dwelt upon with sufficient fulness elsewhere, and is only anxious to provide his readers with a full Algebraical introductory treatise. The first two chapters dwell at considerable length upon Determinants in general; but the third chapter treats much more concisely of the various forms known as Continuants, Alternants, Symmetric and Skew Determinants, Pfaffians and other Determinants. Much of this chapter is the Author's own work; and the whole of it is very suggestive in its treatment. There is apparently much of discovery still in store for the careful worker in this corner of the mathematical field. Ample practice is furnished for the reader in a capital collection of Exercises, to which answers are appended at the end of the book. The fourth chapter contains a slight historical sketch: this is interesting; but we are inclined to regret that regard to space prevented Mr. Muir from extending it. However, he has made some amends by the publication in the 'Quarterly Journal of Mathematics' (Oct. 1881) of a "List of Writings on Determinants" (1693 to 1880), which was originally drawn up for the present work. We have noticed only a few errata and a few obscurities of expression. These can be removed in a second edition, which will no doubt be soon called for. Could not the work then be printed in a form better adapted for the numerous lengthy formulæ?

XVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

May 10, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Relations of *Hybocrinus, Baerocrinus*, and *Hybocystites*." By P. Herbert Carpenter, Esq., M.A.

2. "On the Madreporaria of the Inferior Oolite of the neighbourhood of Cheltenham and Gloucester." By R. F. Tomes, Esq., F.G.S.

3. "On the Exploration of two Caves in the neighbourhood of Tenby." By Ernest L. Jones, Esq.

4. "Note on the Comparative Specific Gravities of Molten and Solidified Vesuvian Lavas." By H. J. Johnston-Lavis, Esq., F.G.S.

From some experiments made on Vesuvian lava, Prof. Palmieri in 1875 expressed the opinion that its specific gravity, when molten, might be as high as 5·0, though when cooled it is only 2·7. The author described the results of experiments made in December 1881 on some lava flowing across the Atrio del Cavallo. Favourable circumstances enabled him to gain a position above a perfectly molten stream, the surface of which was protected from radiation by the heated walls of a tunnel which the lava had already formed by cooling of the crust. Onto this were dropped, from a height of $1\frac{1}{2}$ yard:—(*a*) light scoria; this floated on the surface until lost to view (the stream could be watched for 150 yards or so); (*b*) fairly solid lava, with some vesicular cavities; this slowly sank, until after some distance it disappeared; (*c*) the most compact lava that could be found, in which, however, were a few small cavities; this sank rapidly, the molten rock welling up round it. The author considered that these experiments demonstrate that the cooled lava is more dense than the molten, and that the apparently contradictory results obtained by Prof. Palmieri were due to the fact that the surface of the stream, by loss of heat, had become viscid, so that the solid material floated, though of greater density. The author concluded by citing other confirmatory evidence of his view.

May 24.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Geology of Costa Rica." By George Attwood, Esq., F.G.S., F.C.S., Assoc. Memb. Inst.C.E.; with an Appendix by W. H. Hudleston, Esq., M.A., F.G.S., F.C.S.

The author commenced his journey at the town of Punta Arenas,

on the Gulf of Nicoya. This stands on a peninsula composed of a calcareous sandstone, covered by a dark sand consisting of quartz grains, magnetite, and decomposed felspar and augite. Inland is an igneous rock which occupies, before long, both banks of the Rio Barranca, and on the left bank extends to the sea; it is a greenstone containing porphyritic crystals of augite and triclinic felspar, and appears to contain too much silica for a true dolerite, being rather a representative of one of the more basic forms of the augite-andesites, resembling, in some respects, specimens from the English lake-district described by the late Mr. Clifton Ward. On this rock, after a time, are found boulders of a black augite-andesite; this appears to be identical with the rock found *in situ* in the Aguacate Mountains. Here are gold- and silver-mines, which were described. In the ravine of the Rio Grande lignites are found. Below this is a series of ancient lakes, which on the Pacific slopes have been tapped by the Rio Grande, on the Atlantic by the Rio Reventazon. Here also the country rock is the greenstone already described; and near Cartago there are boulders of trachyte. The volcano of Irazu is a trachyte, probably a quartz-trachyte, forming an important building-stone. Augite-andesites are found at La Palma, about twelve miles N.W. of the volcano. Irazu, a volcano at present passive, but with blow-holes of gas, is between 11,000 and 12,000 feet in height. Turrialba, of about the same elevation, is still feebly active.

The author is of opinion that the filling of the mineral lodes (ancient fissures) in the Aguacate Mountains took place in Tertiary times, probably Pliocene, and that this infiltration was contemporaneous with the eruption of the augite-andesites in the same region. The quartz-trachytes and sandstones are certainly post-Tertiary.

2. "On a remarkable Dinosaurian Coracoid from the Wealden of Brook in the Isle of Wight, preserved in the Woodwardian Museum of the University of Cambridge, probably referable to *Ornithopsis*." By Prof. H. G. Seeley, F.R.S., F.L.S., F.G.S., &c.

3. "On the Newer Pliocene Period in England." By S. V. Wood, Esq., F.G.S. (Concluding Part.)

In this part the author continued, from the first part of the paper (published in the Journal of the Society for 1880), his examination of the conditions which accompanied the emergence of England during the retreat of the ice of the Chalky Clay, and described the damming-up of the valleys which drain to the Wash by that ice after the water-partings between their systems and those of the Severn and Thames had emerged, whereby the fresh water in these valleys was raised, so as to overflow the minor water-partings within their systems and cover them with gravel, such as that at Casewick, within the Welland system (described by Prof. Morris in vol. ix. of the Journal), and those of Cambridgeshire, described by Mr. Jukes-Browne. He referred the freshwater bed at Casewick, covered by this gravel, and the palæolithic brick-earth of Brandon and Mildenhall (which is overlain as well as underlain by the Chalky Clay) to the time immediately antecedent to this—the slight advance

of the ice which thus blocked up and raised the water-line within the systems of the Welland, Nen, Ouse, and Cam having overridden this brick-earth and covered it with the Chalky Clay.

He then described the gravel (*f* of his figures) of the Thames valley, and showed that it was the continuation of the gravel previously described by him as synchronous with the Chalky Clay, and which, as described by him in the first part of his paper, was *over*-lain, and also *under*lain by that clay, it inosculating with those gravels, up the valleys of the Lea and (Middlesex) Colne.

He then described the *Cyrena-fluminalis* formation, which he showed as originating in a depression which raised the water-line in the Thames valley at Grays and Crayford to about 100 feet above the present sea-level, and proportionately higher on the west of London; and described the formation as consisting, at Grays, of four divisions, which in their upward order he called 1, 2, 3, and 4,—No. 1 being the gravel base, No. 2 mostly brick-earth with freshwater shells, No. 3 yellow sand containing freshwater shells in the lower part, but unfossiliferous and false-bedded in the upper, and No. 4, a clay or loam, also unfossiliferous.

These, he showed, are mutually transgressive, both at Grays and at Clacton, No. 3 at Clacton becoming estuarine by the intermixture of marine shells with the *Cyrena*, and No. 4, a loamy gravel which is unfossiliferous, while, from its greater transgression, No. 4 spreads so widely over the gravel *f* that remnants of it occur at Slough, West Drayton, and other places. He then traced the formation northwards in Suffolk; and from the *Cyrena* not being associated there with other than freshwater shells (except at Gedgrave, where the marine shells associated with it are derivatives from the Crag), he inferred that the depression did not bring the sea into Suffolk or East Norfolk. In West Norfolk and around the Wash, however, it did so, the *Cyrena* being associated with the marine gravel at March. The evidences of this depression bringing in the sea around the Wash (which consist of the Nar brickearth and the gravel of Hunstanton, March, and other places in the Fen country with marine shells) extend to about 30 feet elevation. This gravel at Overton, near Peterborough, passes down into a bed with freshwater shells only, thus resembling the Clacton bed, and at March contains the *Cyrena* in abundance. Northwards the formation is represented by the *Cyrena*-gravel of Kelsea Hill, in Holderness; and the evidences of depression rise in that direction to near 100 feet, as a brick-earth, at Kirmington in North Lincolnshire, at between 80 and 90, containing mammalian remains and *Scrobicularia piperata*, with valves united, is regarded by the author as part of the formation; and he instanced the ripple-marked pan beneath this formation at Hessle as evidence of redepresssion or transgression similar to that afforded by the Mollusca at Overton and Clacton. He then described this gravel as extending up the vales of York and Tees to about a similar elevation, and as passing in them, as it does in Holderness, under the clay of the minor glaciation. Southwards he traced the formation as represented by the

shingly sand and gravel of Avisford and Bourne Common in Sussex—the Selsea mud-bed with Lusitanian shells, near the present sea-level, representing the first part of the formation, which the depression carried transgressively to Avisford. In the Thames and lower Lea valleys he described, and showed, by many lines of section, how considerable a denudation accompanied the rise from this depression, so that not only most of the formation but also much of the gravel *f*, of glacial age, on which the uppermost bed of this formation rested, was washed away, the latter having for a great distance been left on an escarpment facing the valley-sides. This denudation, he showed, was in the same places repeated after the formation of the gravel of the minor glaciation.

He then, under another division of the period (which he distinguished as that of the minor glaciation or reindeer age), described the various formations, morainic, atmospheric, fluviatile, and marine, due to a return of glaciation after England had, except in the north-west, become all land. The morainic part in the north-west (which was the Upper Clay of Lancashire and adjoining counties) he regarded as extruded beneath the sea up to that level at which it contains shells, these having been dropped from floe-ice detached from the shores, which drifted over it while thus undergoing extrusion; but in the north-east it was terrestrial, owing to this part having emerged from the depression of the *Cyrena* formation before the moraine reached Holderness, and therefore it contained no shells. The ice giving rise to this moraine was of far less volume than that of the Chalky Clay, and instead of seeking the sea as that did, when the sea lay over the centre and south of England it passed to it in its present position, one stream of it going straight out through the Tees valley, and another down the vale of York and out by the Humber, so as to overspread southern Holderness and the sea-board of Lincolnshire. The fluviatile formation of this minor glaciation is the gravel which overlies the *Cyrena* formation at Crayford and Ilford (Uphallfield) up to the elevation of about 30 feet, and at similar elevation lies up to the foot of that formation at Grays; and it is that which forms the 40–45-foot terrace at Acton, where it has yielded reindeer-remains. Owing to the rise from the depression under which the *Cyrena*-formation accumulated, which had taken place when this gravel was formed, its level does not differ greatly from that of the *fossiliferous* part of the *Cyrena* formation at Grays and Crayford, so that in more inland districts, as at Oxford, the two, though quite different in age, may be undistinguishable. This gravel the author regarded as corresponding in position with the beaches of the buried cliffs of Sangatte, Brighton, Isle of Wight, Portland, and Sili Bay, these beaches and the gravel having originated during a pause in the rise from the depression of the *Cyrena* formation. The floe-ice of this glaciation, driven onto these beaches, left blocks on them, which becoming mixed with loam from rills pouring in summer over the cliffs, is covered by the atmospheric formation which accumulated as, by renewal of emergence, the sea receded from these beaches. At the same time floes grounding on the Pagham

and Selsea flats, which, in correspondence with the shingle of the Isle-of-Wight beach, were then submerged about 30 or 40 feet below their present level, left the great blocks found in the clay-gravel of Pagham and Selsea, which was then forming, and which overlies the mud-bed with Lusitanian shells, and is itself overlain by the atmospheric formation. He also showed by a line of section that this gravel occupies a position several hundred feet below that which the gravel of the great submergence and major glaciation occupies in the adjoining parts of Hampshire.

The atmospheric formation of the minor glaciation he regarded as the brick-earth with angular fragments of stone and splintered flints overlying the buried cliffs and their beaches. This is the "formation of great submergence" (with land shells and Mammalian remains) of Prestwich, and identical with the "warp" of Trimmer and "trail" of Fisher in other parts of England. The origin of this he referred to an annual thawing of the upper layer of the permanently frozen land-surface, such as takes place in arctic countries not occupied by land-ice, such as Siberia. Owing to the subsoil being permanently frozen, no water can penetrate it, so that the thawing surface-layer becomes sludge from the snow-melting and rainfall of summer, and slowly slides from higher to lower places, thus exposing on the higher a continually renewed superficial portion of the permanently frozen soil to this action, and accumulating it in the lower. In sliding, this material has collected not only the bones of animals such as the reindeer and mammoth which lived on this surface, but also those of the hippopotamus, which did not, but had lived during the *Cyrena*-formation stage, from superficial deposits of that stage (from which also they got by derivation into the gravel of this glaciation), in illustration of which he refers to Siberian rivers now receiving the remains of the extinct mammoth and living reindeer alike.

Penetrating fissures in the rocks, this material has formed the amorphous Cave-earth of the districts beyond where the moraine has reached; and the author pointed out that, stalagmite being due to percolation, none could form while the subsoil was thus permanently frozen, which is the reason why the Cave-earth is devoid of it, though always covered by it and sometimes underlain by it, such underlay probably showing that the caves where this occurs were not submerged at the commencement of this minor glaciation.

After giving various reasons which appeared to him to show that the passing away of the minor glaciation took place while Lancashire was still submerged up to an elevation of from 20 to 30 feet, but when the east and south of England was at a somewhat higher level than at present, he described a bed of flattened stones which cover all anterior beds alike in the limestone districts of the south of Lincolnshire, and some gravel with flattened fragments of hard chalk in North Lincolnshire and Holderness, which appear to him to indicate a flooding of the country after the termination of this glaciation. The author then offered some remarks on the coexistence of arboreal vegetation with the land-ice of the first or great glaciation at the

time when it uncovered the plateaux of Norfolk and Suffolk, appealing for the probability of this to the condition of South America, where the inland ice passes in glaciers to the sea in the Straits of Magellan and adjoining channels through dense forests. He also pointed out that the evidences of the Newer Pliocene period, as traced by him, lend no support to the climate-theories of Dr. Croll, Mr. Wallace, or Mr. Murphy, but, on the contrary, conflict with them, as do the respective extensions of the areas of glaciation in Western Europe and Eastern America, while they are equally repugnant to any theory of climate based on changes in geographical conditions; and he concluded by insisting on the British origin of all the ice connected with either glaciation in England, and on the existence of an open north sea throughout.

June 7.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. The President read the following note, forwarded by Don Manuel F. de Castro, Director of the Geological Survey of Spain:—"On the Discovery of Triassic Fossils in the Sierra de Gador, Province of Almeria, Spain."

"The metalliferous limestone of the Sierra de Gador, owing to no fossil remains having been found prior to this occasion, has been a perfect puzzle to all geologists for the last fifty years.

"MM. Maestre, Amar de la Torre, Pernolet, Ansted, and Cooke considered these limestones to belong to the Transition series, the former taking it as a representative of the Mountain Limestones of other parts of Europe. M. Prado hinted that they might be Devonian; whilst M. Willkomm, in the geological map published to accompany his botanical researches in Spain, considered them Silurian. Lately MM. Botella and Vilanova, in their respective maps, have marked them as belonging to the Permian series; whilst M. de Verneuil, coming nearer to the truth, took the whole of the limestones to the south of Granada and the Sierra de Gador as Triassic, though in doubt ("Trias incertain").

"Under these circumstances, I was commissioned by the Director of the Geological Survey of Spain to investigate the S.W. portion of the Province of Almeria, which comprises the Sierra de Gador. In February last I had the good fortune of discovering abundant fossil remains in different parts of the Sierra de Gador, which perfectly fix the age of the metalliferous limestones of this part of Spain.

"The whole series of rocks forming this *sierra*, resting on the mica-schists and slates of the Sierra Nevada, is a succession of black, white, and purple talcose schists at the base, which alternate with some beds of yellowish and porous limestone, and which pass through a considerable thickness of grey limestones and slates, and, precisely where the fossils have been found, to the metalliferous limestone of Sierra de Gador, which appears to form the top of this interesting formation.

"The fossils found belong to the following genera:—*Myophoria*

(*M. lævigata* and *M. Goldfussi*), *Hinnites*, *Monotis*, *Avicula* (*A. Bronni*), *Myacites*, *Rissoa*, and many others difficult to determine.

"The places where the fossils have been found are the following:—on the southern slopes of the Sierra de Gador, in the Rambla del Cañuelo; midway on the road from Felix to Marchal; and in the place named La Solana del Fondon, to the left of the river Andarax, following the track between the mine Sebastopol and the town of El Fondon.

"Joaquin Gonzalo y Xavier."

2. "The Girvan Succession.—Part I. Stratigraphical." By Charles Lapworth, Esq., F.G.S., Professor of Geology in the Mason Science College, Birmingham.

The Lower Palæozoic rocks of the neighbourhood of Girvan, in the south of Ayrshire, have long been famous for the remarkable variety of their petrological features, and for the abundance and beauty of their organic remains; but the strata are so intermingled and confused by faults, folds, and inversions, that it has hitherto been found impossible to give a satisfactory account of the geological structure of the region.

The most remarkable formation in this Girvan area is a massive boulder-conglomerate, several hundreds of feet in thickness, which forms the high ground of Benan Hill, and ranges throughout the district from end to end. Employing this formation as a definite horizon of reference, the author showed, by numerous plans and sections, that it was possible for the geologist to work out the natural order of the strata, both above and below this horizon, and to construct a complete stratigraphical and palæontological scheme of the entire Girvan succession. This succession is composed of the following members, arranged in descending order:—

(I.) Upper Girvan Rocks.

(D.) Dailly Series (1500 to 2000 feet), including the

(3) *Straiton Group*, consisting of grey flags, shales, and grits, with *Beyrichia Klœdeni*, *Cardiola*, &c.

(2) *Bargany Group*, of pale flagstones, shales, and mudstones, with *Retiolites Geinitzianus*, *Cyrtograptus Grayi*, &c.

(1) *Penkill Group*, of purple mudstones, grey flags, and grey-wackes, with *Crossopodia*, *Protovirgularia*, &c.

(C.) Newlands Series (1000 to 1500 feet), embracing the

(3) *Camregan Group* &c., of yellow thick-bedded grits and dark shales, with a band of calcareous rock; abounding in *Pentamerus oblongus*, *Atrypa reticularis*, *Rastrites maximus*, and *Monograptus Sedgwickii*.

(2) *Saugh-Hill Group*, composed of alternations of coarse pebbly grit and zones of grey and black shales, with a coarse conglomerate at the base. Its commonest fossils are *Stricklandinia lens*, *Pentamerus oblongus*, *Favosites gothlandicus*, *Monograptus leptotheca*, &c.

(1) *Mulloch-Hill Group*, formed of shelly sandstones underlain

by a coarse boulder-conglomerate, and containing hosts of Brachiopoda &c., chiefly *Meristella angustifrons*, *Atrypa hemisphærica*, *Nidulites favus*, &c.

(II.) Lower Girvan Rocks.

(B.) Ardmillan Series (1800 to 2000 feet), embracing the

(4) *Drummuck Group* of soft grey mudstones &c., with *Trinucleus seticornis*, *Ampyx*, *Staurocephalus*, *Dicellograptus*, &c.
(3) *Barren or Shalloch Flagstones.*—A great thickness of alternations of grey or green flagstones and shales, generally destitute of fossils.
(2) *Whitehouse Group.*—Purple and green shales and mudstones, striped flagstones and calcareous beds, with *Dionide*, *Dindymene*, *Æglina*, *Agnostus*, *Dictyonema*, *Dicellograptus*, and *Pleurograptus*.
(1) *The Ardwell Group* of dark Graptolitic flagstones and shales, with occasional fossiliferous seams affording examples of *Dicranograptus*, *Leptograptus*, and *Climacograptus*, &c.

(A.) Barr Series (800 to 1000 feet), composed of the

(4) *Balclatchie Beds.*—Highly fossiliferous pebbly grits and nodular shales, with *Lingula Ramsayi*, *L. canadensis*, *Siphonotreta nucula*, *Remopleurides*, *Glossograptus*, &c.
(3) *Benan (or Green) Conglomerate.*—Massive boulder-beds of great thickness, unfossiliferous.
(2) *Stinchar (or Craighead) Limestone Group*, composed of compact limestones, nodular and calcareous flagstones and shales, with *Maclurea Logani*, *Ophileta*, *Orthis confinis*, *Tetradium*, *Didymograptus*, *Clathrograptus*, &c.
(1) *Kirkland (or Purple) Conglomerate.*—Coarse boulder-beds and sandstones, generally of a purple colour.

It was shown that the highest beds of this succession are faulted against strata of Carboniferous age. The discussion of the relationship of its lowest beds to the igneous and metamorphic rocks of Ballantrae was deferred to a future paper. The author pointed out how perfectly this reading of the succession explained the anomalies hitherto supposed to obtain among the fossils of the Girvan region. When the organic remains collected from these strata by previous investigators (notably the magnificent Gray collection) are referred to their natural horizons in this stratigraphical succession, it is found that each of the great petrological divisions of the Girvan series has a collective fauna peculiarly its own, and that the general zoological gradation is identical with that of the acknowledged sequence in the typical Lower Palæozoic areas in Wales and the West of England.

The Lower Girvan rocks are clearly of Ordovician age: while the Upper Girvan rocks as distinctly appertain to the Silurian. The *Barr Series* corresponds to part of the Llandeilo-Bala of Wales: and

the *Ardmillan Series* is of Caradoc -Bala age. The *Newlands Series* answers to the Llandovery formation of Murchison, containing similar fossils and corresponding local breaks in the succession. The *Purple Shales* of Penhill correspond in systematic position, petrological features, and fossils with the Tarannon; and the *Straiton Beds* represent the lower division of the Wenlock.

The development of the palæontological features of the several zones of life in this succession, and the demonstration of their correspondence with the zones already recognized in the synchronous Lower Palæozoic strata of Moffat, the Lake-District, Scandinavia, and elsewhere were reserved by the author for a second part of this memoir.

3. "Notes on the Annelida Tubicola of the Wenlock Shales, from the Washings of Mr. George Maw, F.G.S." By George Robert Vine, Esq.

4. "Description of part of the Femur of *Nototherium Mitchelli.*" By Prof. Owen, C.B., F.R.S., F.G.S., &c.

5. "On *Helicopora latispiralis*, a new spiral Fenestellid from the Upper Silurian beds of Ohio, U.S." By E. W. Claypole, Esq., B.A., B.Sc. (Lond.), F.G.S.

June 21.—J. W. Hulke, Esq., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "On *Thecospondylus Horneri*, a new Dinosaur from the Hastings Sand, indicated by the Sacrum and the Neural Canal of the Sacral Region." By Prof. H. G. Seeley, F.R.S., F.G.S.

2. "On the Dorsal Region of the Vertebral Column of a new Dinosaur, indicating a new genus, *Sphenospondylus*, from the Wealden of Brook in the Isle of Wight. preserved in the Woodwardian Museum of the University of Cambridge." By Prof. H. G. Seeley, F.R.S., F.G.S.

3. "On Organic Remains from the Upper Permian Strata of Kargalinsk in Eastern Russia." By W. H. Twelvetrees, Esq., F.G.S.

4. "The Rhætics of Nottinghamshire." By E. Wilson, Esq., F.G.S.

During the last few years several sections of the Rhætic beds, in addition to those already known near Gainsborough and Newark, have been exposed in making railway-cuttings. These were described by the author. The beds usually assigned to the Rhætic consist of:— Lower Rhætic, greyish or greenish marls; Middle or *Avicula-contorta* series; and Upper Rhætic (white Lias), a series of variable shales and light-coloured limestones. The author pointed out that in the Nottinghamshire district there is always a clear line of division, and sometimes indications of erosion, between the *Avicula-contorta* series and the so-called Lower Rhætic beds, while the latter graduate down uninteruptedly into the Upper Keuper Marls. Further, they

are practically unfossiliferous, while in the other series there is evidence of marine life and the remains of a fauna ranging upwards into the Lias. Hence the author proposed to class these Lower Rhætics with the Trias, and regard the Rhætic series as commencing with the base of the *Avicula-contorta* group.

5. "On the Silurian and Cambrian Strata of the Baltic Provinces of Russia, as compared with those of Scandinavia and the British Islands." By Dr. F. Schmidt. Communicated by Dr. H. Woodward, F.R.S., F.G.S.

The Cambrian and Silurian strata in question are found stretching over an area 400 miles long by 80 miles wide. The country occupied by these strata is a nearly uniform plain covered by glacial deposits; but sections are presented by the sea-cliffs, which are from 90 to 150 feet high. The strata consist mainly of marls and limestones, arenaceous deposits being rare; and they form a continuous series from the base of the Cambrian to the top of the Silurian, the whole of these strata being in conformable succession and unconformably overlain by the Devonian. Although the representative of the Cambrian or Primordial Silurian contains neither *Paradoxides* nor *Olenus*, nor, indeed, any Trilobites whatever, but only Lingulidæ and Graptolites, yet its stratigraphical position leaves no doubt as to its age. The Lower Silurian or Ordovician is the richest of the divisions, the strata of this age forming a perfectly continuous series. The author divides these beds into the stages B, C, D, E, and F, several of which are divisible into substages. Of the Stage B the lowest bed, 1 to 10 feet thick, consists of the Glauconite-bands containing the casts of Foraminifera described by Ehrenberg, which correspond to the *Ceratopyge* stage of the Scandinavian geologists. Above this, and closely connected with it, is the Glauconite-limestone, from 12 to 40 feet thick. Next comes the famous *Orthis*-limestone, a thin bed with a very interesting fauna, corresponding to that of the *Phyllograptus*-schists. The author's beds C, D, E, and F can be paralleled with the strata of Scandinavia, but have no exact representatives in the British Islands. The stage E appears to represent the Bala of England or the Trenton of North America. Although there is no stratigraphical break, there is a marked palæontological division between the Upper and Lower Silurian, there being no strata of intermediate age represented. The Upper Silurian is divided by the author into the stages G, H, I, K, which can be exactly correlated with the strata of Scandinavia. The stages G, H do not appear to have precise representatives in the British Islands; but I is undoubtedly equivalent to the Wenlock, and K to the Ludlow. In the Baltic provinces there are no representatives of the passage-beds and Lower Devonian, but the Silurian strata are unconformably covered by the Old Red Sandstone with *Coccosteus*, *Asterolepis* *Bothriolepis*, *Homosteus*, and *Heterosteus*, there being a marked overlap between the two series. The author argues in favour of considering the Cambrian, Ordovician, and Silurian as forming, in the Baltic provinces, one "system." The author is able to construct a

section connecting the Silurians of Russia and Scandinavia, and passing through the islands of Gothland and Oesel. The paper is accompanied with a map and a tabulated list of the fossils found at the several horizons which have been distinguished by the author. He points out which of these species are found ranging into other areas, and proposes eventually to publish figures and descriptions of the characteristic Russian forms. The first part of the author's palæontological work has just appeared in the shape of a memoir describing 60 species of Trilobites of the genera *Phacops*, *Cheirurus*, and *Encrinurus*. The total number of species of the Trilobites is about 150, of which only about 15 occur in the Upper Silurian.

6. "On Chilostomatous Bryozoa from Bairnsdale (Gippsland)." By A. W. Waters, Esq., F.G.S.

7. "The Silurian Species of *Glauconome*, and a suggested Classification of the Palæozoic Polyzoa." By G. W. Shrubsole, Esq., F.G.S., and G. R. Vine, Esq.

8. "On the Cause of the Depression and Re-elevation of the land during the Glacial Period." By T. F. Jamieson, Esq., F.G.S.

The author commenced by noticing the theory advanced by Adhémar and Croll, according to which the submergence was due to the effect of a polar ice-cap causing a displacement of the earth's centre of gravity and thereby drawing the ocean towards the ice-covered pole, and proceeded to show that this theory is opposed to the geological evidence, according to which the amount of submergence has been unequal in adjacent areas and along the same parallels of latitude, showing that the movement has been in the land and not in the sea. The facts of submergence also prove that no such cap of ice could have existed at the time in the northern regions. Sundry other objections were also pointed out. The author then went on to state his own hypothesis, which is to the effect that the depression of the land was caused by the weight of ice laid upon it, and the re-elevation by the disappearance of the ice. The amount of depression would depend partly on the weight of ice and partly on the elasticity or yielding nature of the ground beneath it. He then proceeded to consider what was the weight of ice that probably existed, and referred to the elastic and flexible nature of the earth's crust, as evinced by earthquakes &c.

He further considered the relation of time to pressure, and touched upon the probable rate of subsidence, which he supposes to have been very slow and gradual. The recovery of level, he thinks, would also be very gradual, and probably, in most cases, not complete.

He next proceeded to show how his hypothesis is borne out by an appeal to geological evidence in various countries, taking England, Ireland, North America, and Greenland as examples. He further pointed out its application to the facts connected with the loess beds, Fjord latitudes, and lake-basins, and concluded with some observations on the remarkable connexion between glaciation and submergence in all countries.

XVIII. *Intelligence and Miscellaneous Articles.*

ON SOME EXPLOSIVE ALLOYS OF ZINC AND THE PLATINUM METALS. BY H. SAINTE-CLAIRE DEVILLE AND H. DEBRAY.

SOME time before the sickness of my dear and illustrious master Henri Sainte-Claire Deville, we had undertaken to return to some points in order to complete our old researches respecting platinum. Our work, as regards the division of the osmides, was almost finished, when it was interrupted by his sickness and death. I have had to terminate it; and today I present the result to the Academy.

It is known that the osmides cannot be divided by mechanical action. If for example, one essays to pound them in a tempered steel mortar, the osmide of iridium penetrates into the material without being either blunted or broken.

If, however, it be fused with 25 or 30 times its weight of zinc, and if after the mixture has been kept for some hours at an incipient red heat it then be more heated in order to volatilize all the zinc, there remains a spongy mass, easily divided, and, in this state, completely and with facility attacked by the mixture of nitrate and binoxide of barium which we have employed for oxidizing the two metals of the osmide and to render them soluble in acids. As it is easy to eliminate baryta from its solutions, the analysis of the osmide then becomes possible.

What is the part played by zinc in this division? by what mechanism is it effected? are questions which we had not then examined, contenting ourselves for the moment with a practical result sought in vain by Berzelius, which facilitated our entering upon that study of the platinum metals which has occupied us for many years.

I. Some osmide of iridium is thrown into some zinc heated to dull redness and which has been previously well cleaned with sal-ammoniac. A brisk disengagement of heat is produced. The mass is kept melted at this temperature during five or six hours, in order to attack completely the large grains of osmide. The cold button is then dissolved in dilute hydrochloric acid. The solution of the zinc takes place with great violence; and there remains a blackish residue having the appearance of graphite, which contains all the noble metals of the osmide. The greater part of the iron existing in this material is dissolved at the same time as the zinc; on the other hand, there remains a notable proportion of zinc combined with the metals of the osmide, and it is not removed by prolonged contact with concentrated hydrochloric acid.

This residue, well washed, and dried at 100°, diffuses in the air a slight odour of osmic acid. Heated to nearly 300° it suddenly ignites, almost with explosion, spreading fumes of zinc and abundant vapour of osmic acid. As this deflagration took place *in vacuo*, without any sensible liberation of gas, and of course without the

production of oxide of zinc and osmic acid, we must conclude that at about 300° the substance undergoes a change of state accompanied by a large disengagement of heat. In air the change of state is immediately followed by combustion, which augments the evolution of heat.

The residue is only partially attacked by concentrated nitric acid or aqua regia. It is quickly and completely oxidized when thrown into a mixture of fused potass and potassium nitrate; ir must even be thrown in in small portions, the mixture being but little heated, to avoid too strong a deflagration when the divided material touches the surface of the oxidizing liquid. If one wishes to make use of this residue either for the analysis of the osmide or to extract the metals which it contains, it is better to mix it with anhydrous baryta and barium nitrate: one can then heat it without fear of losing the substance, which is then attacked totally and easily.

This substance is evidently a complex mixture of various alloys which zinc is capable of forming with the platinum metals; we were therefore induced to examine more closely those different combinations.

II. Osmium simply dissolves in zinc. When the fused button of this metal with osmium in a state of division is acted upon by hydrochloric acid, there remains pure osmium with a crystalline appearance.

Palladium and platinum, treated in the same manner, leave a residue consisting of alloys which undergo no isomeric modification when heated *in vacuo*. Rhodium, on the contrary, and iridium and ruthenium especially, as one of us has already proved *, combine with zinc with much liberation of heat; and when the zinc button is dissolved in hydrochloric acid, residues are obtained which are susceptible of undergoing a true isomeric modification accompanied by a brisk liberation of heat, without loss of gas, when heated in a vacuum above 300°. Before the liberation of heat, the blackish residues would be more or less readily attached by aqua regia. They lose this property after the liberation of heat, and then take the metallic appearance.

The thermal phenomenon which accompanies the change of state of the iridium residue is so marked that it may serve for the recognition of the presence of small quantities of iridium in platinum (1 or 2 per cent. for example). The metal to be assayed is dissolved in a large excess of zinc; and the button is acted upon by dilute hydrochoric acid; the residue, well dried, raised to a temperature above 300° in a platinum capsule becomes incandescent at various points. Ruthenium and rhodium produce similar effects.

III. In brief, osmium is the only platinum metal which does not retain zinc when its alloy with a large excess of zinc is treated with an acid capable of dissolving that metal. The other metals obstinately retain a notable proportion of it (on the average 10 to

* *Comptes Rendus,* t. xc. p. 1150.

12 per cent.); and the metals which are insoluble in aqua regia (rhodium, iridium, and ruthenium) then remain in the state of peculiar products, without metallic lustre, which seem to be an allotropic modification of the true alloys possessing a metallic appearance.

The action of zinc upon the osmide of iridium is thus accounted for naturally. If the heat liberated in the combination of iridium and zinc much exceeds the heat of combination of osmium and iridium, the osmide, in conformity with the laws of thermochemistry, will be destroyed by the zinc; the osmium dissolves and may crystallize in the excess of metal; the iridium and the other metals remain combined with the zinc. It is the residue from the action of the hydrochloric acid upon this alloy with excess of zinc that constitutes the explosive substance above considered. In fact the heat liberated in the union of zinc with iridium, ruthenium, and rhodium, which are the dominant metals of the osmide, is truly enormous: on adding, for instance, 1 part of iridium to 30 or 40 per cent. of melted zinc at a temperature below a red heat, there is combination accompanied by actual incandescence of the whole mass of metal; it is the same with the two other metals. If all the zinc be driven out by heat (as was done by us in our old experiments), the hardly fusible metals of the osmide, free or alloyed, remain in a state of extreme division, in which they are much more readily attacked than the natural osmide.

In an early Note we shall speak of facts observed in the solution of the platinum metals in copper.—*Comptes Rendus de l'Académie des Sciences*, June 12, 1882, t. xciv. pp. 1557–1560.

ON THE REACTION-CURRENT OF THE ELECTRIC ARC. BY M. JAMIN, WITH THE ASSISTANCE OF M. G. MANEUVRIER.

The two currents, in alternately opposite directions, given by Gramme's self-exciting machine are absolutely equal; consequently they do not decompose water, and a tangent-compass intercalated in the circuit undergoes no deflection, since the contrary effects following at very close intervals destroy one another. This destruction of the effects is kept up when one or several burners are put into the circuit, provided that the two carbons are equal, disposed in exactly the same manner, and are heated equally.

If eight or ten Bunsen elements be introduced into the total circuit, they communicate to the compass a deflection δ when the machine is at rest, and a deflection δ', absolutely equal to δ, when the machine is working.

	δ.	δ'.
First experiment	32	33
Second experiment........	38	38·45
Third experiment	33	34·10

This equality proves that the resistance of the wires of the machine does not vary, whether the machine be at rest or in motion;

it proves also that the two effects of the machine and the battery are superposed and independent.

If we now suppress the battery, but ignite in the circuit a burner formed of two unequal carbons—one stout (0·004 metre), the other thinner (0·002 metre),—this asymmetry suffices to determine a permanent deflection of the compass, just as if a battery had been intercalated. The two systems of mutually inverse currents given by the machine cease, therefore, to be equal; that which is directed from the thick carbon to the thin, from the less hot part to that which is more so, prevails over the system of which the direction is opposite. A differential current results, indicated by the compass, and the more intense as the difference of thickness of the two carbons is more marked; it reaches its maximum between a large mass of retort-carbon, which becomes little heated, and a crayon terminating in a fine point, which attains the highest temperature. The same phenomenon is obtained, and in the same sense, when an arc is produced between a mass of any metal and a carbon point. As it is difficult to maintain the constancy of the arc, the intensity of the differential current is very variable; the following results must be regarded as only a first approximation:—

Lead.	Iron.	Carbon.	Copper.	Mercury.
29°	30°	31°	60°	70°

In general the differential current is weak or none when the arc is but of little extent; it increases with the distance of the electrodes. For zinc it is at first as intense as with copper; but it falls suddenly, probably on account of the oxide with which the metal is soon covered.

The deflection depends on two things:—1, on the mean electromotive force of the differential current; 2, on the resistance introduced into the current by the arc which is formed. It is easy to compare that force and that resistance in the different cases in the following manner:—

We introduce into the total circuit a battery of n pairs, having an electromotive force $n\mathrm{A}$. According to whether it acts in the direction of the differential current or in the opposite direction, we have

$$i=\frac{x+n\mathrm{A}}{\mathrm{R}}, \quad i'=\frac{x-n\mathrm{A}}{\mathrm{R}}.$$

When x is greater than $n\mathrm{A}$ both deflections have the same sign, and we find

$$\frac{i}{i'}=k=\frac{x+n\mathrm{A}}{x-n\mathrm{A}}, \quad x=n\mathrm{A}\frac{k+1}{k-1};$$

but if x is less than $n\mathrm{A}$, the two deflections have opposite signs—

$$\frac{i}{i'}=k=\frac{x+n\mathrm{A}}{n\mathrm{A}-x}, \quad x=n\mathrm{A}\frac{k-1}{k+1}.$$

I employed fresh and well amalgamated Bunsen elements; the experiment, not susceptible of great precision, on account of the variations of the arc, gave the following values of the electromotive force x of the differential current evaluated in Bunsen elements:—

Lead.	Iron.	Carbon.	Copper.	Zinc 1.	Zinc 2.	Mercury.
2·5	3·2	5·0	50·6	66·2	5·7	103·7

The first three substances are nearly equal; the last three are very active. Copper is equivalent to 50 bunsens; zinc to 66 at the commencement of the action, falling to 5. As to mercury, it presents an enormous and very constant value; the differential current has a mean electromotive force equivalent to 103·7 bunsens. The same determinations conduct us to an estimation of the introduced resistance. We have, in fact,

$$i-i'=\frac{2nA}{R}, \quad R=\frac{2nA}{i-i'}.$$

R is in the inverse ratio of $i-i'$.

Now the total resistance R was composed (1) of that of the wires of the machine, (2) of that of the battery, (3) of that of the electric arc. The last alone is variable, and increases or diminishes the value of R; it will therefore be the more the less $i-i'$ is. Here are the calculated values of $i-i'$:—

Carbon.	Iron.	Lead.	Copper.	Mercury.	Zinc 1.	Zinc 2.
0·406	0·307	0·283	1·41	0·89	1·02	0·56

It would follow from these numbers that carbon, iron, and lead offer the greatest resistance, copper and mercury the least.

The differential current can only be explained in two ways—either by a difference in the resistance, or else by inequality in the inverse reactions of the arc in the one direction or the other.

In order to ascertain if the resistance of the arc varies with the change of direction, I caused a continuous current to pass, first from the carbon to the mercury, afterwards from mercury to carbon. A compass, placed in derivation, measured the intensity in the two cases. I could not measure any appreciable difference. But these experiments presented a remarkable peculiarity: when the current passed from mercury to carbon, the arc had a very pronounced green colour, and the volatilization of the metal proceeded vigorously; in the contrary case the arc was reddish, and there was a less abundant production of vapour. This renders evident the asymmetry existing in the two cases. Now, when the alternating currents of a Gramme machine are directed through this burner, the arc is green, which proves that the current going from mercury to carbon predominates over that which goes in the opposite direction; and as there is no difference in the resistance of the arc, it is

in a peculiar property of the alternating currents that the reason of the differential current must be sought.

Each of the two systems of currents stores up, at the instant of its commencement, a certain sum of energy, which is set free when it ceases, and is manifested by a contrary current, or, as Edlund says, by an inverse electromotive force. Thus a first current →, initially very feeble, gradually increases, and, when it ceases, gives rise to an inverse reaction ←, which adds itself to the current ← developed by the machine at the same instant. If, then, one of the systems of current → presents a weaker reaction than the contrary system ←, it will be less weakened and more reinforced, and will determine the direction of the differential current.

Whatever may be the explanation of these facts, it is clear that, once produced by an arc with mercury, the differential current entirely changes the action of the machine, that one of the systems of currents is, if not extinguished, at least considerably weakened, and that the other system is constituted by successive currents of greater intensity and duration. Also every additional electric arc introduced presents the same aspect as that of the batteries—that is to say, greater brightness and heat at the positive pole, with transfer of matter to the negative pole. The machine, previously incapable of decomposing water, becomes capable of decomposing it like a battery with an electromotive force equal to 100 Bunsen pairs; it can, like the batteries, determine all the chemical actions we will, magnetize soft iron, reduce metals, convey force—in a word, replace a continuous-current machine in its applications.

There are two types of magneto-electric machines: those of the one kind, derived from the Gramme system, can give directly currents constant in direction; the others, like those of Nollet or Méritens, can engender only alternating currents: the latter are applicable only to the production of light; it has been in vain attempted to employ them for chemical operations by directing the currents with a commutator. It is evident that this commutator might be replaced automatically by one or more arcs formed between a bath of mercury and a carbon point*. It remains to ascertain what are the economical conditions of that transformation.—*Comptes Rendus de l'Académie des Sciences*, June 19, 1882, pp. 1615–1619.

ON THE MOTION OF A SPHERICAL ATOM IN AN IDEAL GAS. BY G. LÜBECK †.

The author considers an atom of mass M, moving through a gas at rest, of which the atoms are of mass m. With respect to the impact, he avails himself of the principle of *vis viva*, of that of the centre of gravity, and, lastly, of the principle that communication

* These experiments were made at the laboratory of the Sorbonne.
† *Festschrift des Fried.-Werd.-Gymn. Berlin*, 1881, pp. 295–312.

of motion takes place only in the direction of the common normal to the two cylindrical atoms at the instant of the impact. The atoms then behave like perfectly hard elastic spheres; no internal motion takes place. First the number $\mathfrak{N}$ of the collisions is calculated (in the known manner) which the atom M experiences when during a unit of time it moves through the atoms m with the constant velocity Ω; and it is remarked that $\mathfrak{N}$ is a minimum for $\Omega=0$.

It is then assumed that the velocity of M is continually altered in quantity and direction by the collisions, but at the same time a certain mean velocity A, in the direction OX, prevails. Those deviations from the mean motion effected by the impacts the author names the "oscillating motion" of the atom M. [To some extent in this way behaves an atom of one kind of gas which is diffused with a certain velocity through another.—THE REPORTER.] The probability f_ν that the atom M has the velocity-components U_ν, V_ν, W_ν in the directions of the axes of coordinates the author finds by a method first employed by O. E. Meyer. He first finds the probability that the atom, in n arbitrarily chosen time-elements, has successively the velocity-components

$$U_1, V_1, W_1, U_2, V_2, W_2, \ldots U_n V_n W_n$$

equal to the product

$$f_1, f_2 \ldots f_n.$$

As the most probable distribution of velocities he designates that for which this product is a maximum. But now the sought-for function f is not variated, but the differential quotients of the above product with respect to the variables contained therein are, under the corresponding accessory conditions, put equal to 0, which gives

$$f=ce^{-\lambda[(U-\alpha)^2+(V-\beta)^2+(W-\gamma)^2]}.$$

By F the author denotes the ratio of the time during which the velocity-components of the atom M lie between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$ to the whole time of the motion of that atom; and he finds from the above, putting

$$U=\Omega\cos\theta', \quad V=\Omega\sin\theta'\cos\phi', \quad W=\Omega\sin\theta'\sin\phi',$$

$$\omega=\sqrt{km}\,\Omega, \quad a=\sqrt{km}\,A, \quad \kappa=\frac{\lambda}{km},$$

$$F=\left(\frac{\kappa}{\pi}\right)^{\frac{3}{2}}e^{-\kappa(\omega^2 a\omega-2\cos\theta'+a^2)}\omega^2\,d\omega\sin\theta'\,d\theta'\,d\phi'.$$

For the quiescent gas, Maxwell's distribution of velocities is assumed:—the number of the atoms in the unit of space for which the X component of the velocity lies between u and $u+du$ is

$$N=\sqrt{\frac{km}{\pi}}\,e^{-kmu^2}\,du.$$

If L be the mean *vis viva* of the atom M, this gives for the mean *vis viva* $L-\frac{1}{2}(MA^2)$ of its oscillating motion the value $3M/4\lambda$. The mean *vis viva*, however, of an atom m is equal to $3/4k$. In order to find the dependence of L on A, the author seeks the probability $\Phi dU_1\,dV_1\,dW_1$ that, if the atom M before the impact had the velocity-components U, V, W, the resulting impact is exactly such that the velocity-components after it lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1. Considering first an impact occurring upon any surface-element of the sphere, and then summing all the collisions resulting upon all the surface-elements so that the condition mentioned is fulfilled, he finds

$$\Phi dU_1\,dV_1\,dW_1=\left(\frac{M+m}{2m}\right)^2 R^2 N\sqrt{\frac{km}{\pi}}$$

$$\epsilon^{-km\left[\frac{M+m}{2m}Q+\frac{U(U_1-U)+V(V_1-V)+W(W_1-W)}{Q}\right]^2}\frac{aU_1\,dV_1\,dW_1}{Q\mathfrak{N}};$$

in which R is the radius of the atom M,

$$Q=+\sqrt{(U_1-U)^2+(V_1-V)^2+(W_1-W)^2}.$$

Now, as the atom M in the course of the time-unit collides FN times with m so that before the impact the velocity-components of the former lie between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$ without any further condition,

$$F\mathfrak{N}\Phi dU_1\,dV_1\,dW_1$$

is the number of impacts which M during the unit of time suffers in such wise that the velocity-components before the impact lying between the limits U and $U+dU$, V and $V+dV$, W and $W+dW$, after the impact lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1. If we integrate over all the differentials contained in F, we obtain the number of impacts after which the velocity-components lie between the limits U_1 and U_1+dU_1, V_1 and V_1+dV_1, W_1 and W_1+dW_1 without any other condition; and since this must be equal to the number of impacts at which the velocity-components before the impact lie between the same limits, without any condition for those after the impact, we get

$$F_1\mathfrak{N}_1=dU_1\,dV_1\,dW_1\int_0^{\infty}\int_0^{\pi}\int_0^{2\pi}F\mathfrak{N}\Phi.$$

If in this equation, which holds for all values of U_1, V_1, W_1, we assign to each of these quantities the value 0, we get, after carry-

ing out the three integrations,

$$\left(\frac{2m}{M+m}\right)^2=\frac{ep^2}{\eta p}\int_0^p e^{-x^2}\,dx,$$

in which

$$p^2=\frac{\kappa^2a^2}{\kappa+\left(\frac{M-m}{2m}\right)^2},\quad \eta=\kappa+\left(\frac{M-m}{2m}\right)^2.$$

From this we find

$$\kappa>\frac{M}{m},\quad \frac{3M}{4\lambda}=L-\frac{M}{2}A^2<\frac{3}{4k};$$

that is, the part of the mean *vis viva* expended upon oscillating motions of the atom is in general less than, and for $A=0$ is equal to the mean *vis viva* of an atom m. For $A=\infty$ it is equal to 0. The author demonstrates also that

$$L-\frac{M}{2}A^2$$

with A increasing must always constantly diminish.

By integrating $F\mathfrak{N}$ over the three differentials therein contained, the author finds the number Z of the collisions which the atom M suffers in unit time, and by threefold integration of $F\,.\,\Omega$ the sum S of all the lengths of path of the atom during the unit of time. $\rho=S/Z$ is the mean path between two collisions. Simple values were obtained only for $A=0$.

Since the atoms m form a resting gas, it is clear that the atom M will continually lose more and more of its own proper velocity through the collisions. The author calculates, first, how much a collision of any kind changes the X component of the velocity of M. This quantity, multiplied by the number of collisions of that kind in unit time, and integrated over all kinds of collisions, gives the diminution dA/dt which the proper velocity of the atom M undergoes on the average in the unit of time, and which the author, like Stefan (in his theory of gas-diffusion), designates the resistance of the gas to the atom M. If A is very small, the calculation gives

$$\frac{dA}{dt}=-q^2A,\quad A=A_0e^{-q^2t},$$

while

$$p^2=\frac{\sqrt[4]{\pi}NR^2}{\sqrt[3]{kM}}\cdot\frac{M}{m}\frac{1+\frac{13}{4}\frac{M}{m}+4\frac{M^2}{m^2}+\frac{M^3}{m^3}}{\left(1+\frac{M}{m}\right)^{\frac{7}{2}}}.$$

—Wiedemann's *Beiblätter*, 1882, no. 6, pp. 451–455.

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XIX. *On the Electricity of Flame.* *By* JULIUS ELSTER *and* HANS GEITEL*.

[Plate IV. figs. 1–4.]

§ 1. *Introduction.*

ON the electricity of flame there is already a long series of memoirs; but in many respects they contradict one another, both in regard to the results and also to the views advocated by the different authors as to the cause of flame-electricity. As Holtz† has briefly given a very perspicuous digest of all the memoirs which refer to the electrical behaviour of flames, a reiterated historical quotation of them in this place may certainly be dispensed with.

The origin of the electrical difference can be accounted for by the following three causes:—

(1) The electricity of flame is caused by the process of combustion as such (Pouillet‡, Hankel§).

(2) It arises from the flame behaving to the metals introduced as electrodes like an electrolyte (Matteucci‖). To this explanation, evidently, no other meaning can be attached than that the different layers of the flame excite differently by contact the wires immersed in them. For shortness, we will

* Translated from Wiedemann's *Annalen*, 1882, no. 6, vol. xvi. pp. 193–222.

† Carl's *Rep.* xvii. pp. 269–294 (1881).

‡ *Ann. de Chim. et de Phys.* xxxv. p. 404 (1827).

§ Pogg. *Ann.* lxxxi. p. 212 (1850).

‖ Phil. Mag. 1854, viii. p. 309.

in future designate this as the "electrolytic" theory, and, in correspondence therewith, speak also of "electrolytic" excitation by the flame.

(3) Its explanation is found in a thermoelectric difference of the electrodes (Buff)*.

The theories which are deduced from the unipolar conductivity of flame we may be permitted to pass without notice, since, as will result from the following investigation, sources of error may have prevailed here, causing the conclusions drawn from them to appear doubtful.

Besides these differences in respect of theory, however, the different observers also adduce experiments which are absolutely irreconcilable with one another. This goes so far that even in regard to the question whether the positive or the negative electricity is that which is proper to flame no unity prevails.

The reason of this lies in the fact that all the observers have overlooked a point that plays a very essential part in the electricity of flames, namely the behaviour of the shell of air which immediately envelopes the flame.

The maximum of electromotive force is always found when one wire is introduced into the latter, and another into the interior of the flame, as will be shown in the following. At the same time, however, with the electrodes in this position the resistance of the hot layer of gas separating the wires is uncommonly greater; so that we can hardly reckon on measuring the intensity of the current with the aid of a multiplier; it is perhaps a consequence of this circumstance that all the observers who have investigated flame-electricity by means of that instrument have lost sight of the point above named.

As hitherto, so far as is known to us, the electricity of flame has not been examined with an electrometer permitting exact measurements, it appeared to us of importance to test the electric behaviour of flame with Thomson's quadrant-electrometer, and eventually to verify the correctness of one of the theories above cited.

§ 2. *Apparatus and Method.*

In order to keep the charge of the needle of the electrometer constant, it was connected with one of the poles of a Zamboni's battery consisting of 2400 pairs of plates, the other pole of which was led to earth. The double deflection produced by a normal daniell varied 4 or 5 scale-divisions during

* Lieb. *Ann.* lxxx. p. 1 (1851) & xc. p. 1 (1854).

the time of the investigation, and amounted in the mean to 112. To this normal daniell, putting its electromotive force $=100$, all the data given in the following communication are referred. The scale was placed at 2 metres distance from the mirror; and the deflection left and right from the position of rest was measured by means of a suitable turn-plate.

In order to convey the electricity of the flame to the quadrants, of which one was, as usual, connected with the earth, straight wires were mostly employed, or electrodes from liquids, which, fixed in suitable stands well insulated upon cakes of resin, could be raised and lowered at pleasure.

The experiments were made with well-insulated Bunsen burners and with alcohol flames. The flames of the former issuing from apertures of the usual width proved too flickering, and therefore the measurements too uncertain. On this account a very small Bunsen burner was prepared from a glass tube of 4 millim. width. Its upper extremity was surrounded by a platinum sheath, in order to avoid colouring the flame by the gradually heating glass. When one electrode [the "base-electrode"] (B, fig. 1) was immersed in the foot of the flame, and the other in its apex [the "apex-electrode"] (S, fig. 1), with the turn-plate in one position the foot of the flame was connected with the earth, and the apex insulated; with the other position the reverse took place.

§ 3. *Longitudinal Polarization of the Flame.*

Hankel states that when one platinum wire is introduced into the apex of a flame and one into its base, a galvanometer indicates an electric current passing from above downwards. From this it might be inferred that the flame is polarized lengthwise.

The corresponding experiment with the electrometer gives apparently the same result; but in this case two very striking points are to be remarked.

The first point is this:—If the experiment be arranged as represented in fig. 1, the apex mostly appears, as in Hankel's experiment, negative to the base; but very often, and apparently without any external cause, the reverse takes place.

The second is that when the metal from which the flame issues is connected with that quadrant which is conducted to earth, while the insulated electrode is introduced at different heights *s* above the base, by suitably shifting the insulated electrode within a cross section the appearance of a constant potential within the flame is easily attained.

Thus the electromotive force (E) was determined as follows:—for

millim.	
$s = 1$	$E = 104$
$s = 20$	$E = 104$
$s = 60$	$E = 94$ (wire at the extreme apex).

Lastly, as a third noteworthy point may be added that the electromotive force is independent of the size of the flame, and, consequently, of the amount of the burning gas. This is evident from the following experiment:—

By regulating the admission of the gas, over the same aperture of the burner three flames of different height h were produced, and their apices connected with the insulated pair of quadrants by a clean plate of platinum. There were found, for

$h = 20$	$h = 35$	$h = 70$
$E = 73{\cdot}7$	$E = 75$	$E = 73{\cdot}9$,

consequently the electromotive force E independent of the height h of the flame. Two subsequent series of experiments gave the same result. Here the arrangement of the experiment was that shown in fig. 2 *a*. There were found, for

	1st series.	2nd series.
$h = 20$	$E = 142$	$E = 213{\cdot}2$
$h = 40$	$E = 145$	$E = 219{\cdot}2$
$h = 70$	$E = 142$	$E = 216{\cdot}0$

The reason of the value of E being here so much higher will appear subsequently.

§ 4. *Polarization of the Flame in the Cross Section.*

If the flame were polarized lengthwise, the surfaces of equal potential would be given by planes perpendicular to the axis of the flame. On examining cross sections of this sort, the surprising result was obtained that, if the two platinum wires laterally introduced penetrate the flame to an equal depth, the difference of potential within one and the same cross section $= 0$, but that with a slight horizontal displacement of either electrode a difference of potential often appears, which equals that produced by the vertical displacement or far exceeds it. The latter takes place whenever one of the electrodes no longer dips into the flame, but into the enveloping shell of hot air (A A′, fig. 1). The layer of air immediately enveloping the flame plays therefore an essential part in the electrical behaviour of the flame.

Hence, in order to avoid possible errors, it appeared advisable to cover as much of the electrodes as was not within the flame with an insulating coat, which could easily be done by fusing the platinum wires into glass tubes. The wire projecting out of the glass was just long enough to reach from one margin of the flame to the other.

If, now, two such platinum wires were placed opposite one another in one and the same cross section of the flame, and one of them was continually moved further and further from the other, with its complete withdrawal from the flame a very considerable increase of the electromotive force occurred; it rose from 12 to 192; and the electrode which was in the layer of hot air was positive.

Accordingly, a flame gives the maximum of action when the apex-electrode is introduced into the hottest part of the flame, and the base-electrode into the sensitive hot layer of air, about as represented in fig. 2 *a*.

Let it be further remarked that with this arrangement of the experiment a reversal of the polarity of the flame was never observed by us, and that all carburetted-hydrogen flames exhibited the same behaviour.

§ 5. *On the Change of the Polarity of a Flame.*

It was mentioned in § 3 that one and the same flame appears sometimes positive, sometimes negative. Since, then, the wire introduced into the air envelope becomes highly positively electric, it is clear that uncommonly much will depend on how the electrodes are introduced into the flame. If it is wished to have the apex of the flame negative, the apex-electrode must be completely enwrapped in the flame; in the other case, especially if the base-electrode dips quite into the flame, the electric excitation of the enveloping air stratum may preponderate, and consequently the flame appear positive. If this is the true explanation, it must be possible artificially to change the polarization of a flame.

Of the experiments with alcohol-flames we note the following; the positions of the platinum electrodes are given in fig. 2 *a*, *b*, *c*, *d*, *e*. In fig. 2 *a* the base-electrode B is in air at about $\frac{1}{2}$ millim. distance from the margin of the flame, and will now be gradually pushed in till (fig. 2 *c*) it touches both margins of the flame. The apex-electrode S has had this same position from the beginning. The electrode B was then left in this situation and S gradually drawn out of the flame until it was all in air (fig. 2 *e*). The deflections were as follows:—

Position of rest of the electrometer-needle: 511·0.

Daniell = 100.

Position of electrodes.	Orientation of needle.	Electromotive force.
a	439·0	+144
b	485·0	+ 52
c	510·5	+ 1
d	547·0	− 72
e	572·0	−122

(The sign placed before E gives the direction of the electric excitation of the electrode B.)

Or, in words:—

As long as B is outside of the flame it is positive, S negative (fig. 2 *a*).

When B is in the margin of the flame, the deflection diminishes, but B still remains positive, S negative (fig. 2 *b*).

When B and S are equally immersed in the flame the needle returns to its resting-position (511); deflection 510·5. Therefore, with the position fig. 2 *c*, E = 0.

If S is now drawn back into the margin of the flame (fig. 2 *d*), it becomes positive, B negative; the polarity of the flame is therefore reversed. And when, finally, S is quite outside of the flame (fig. 2 *e*), it is strongly positive; consequently it behaves exactly as did the electrode B in the initial position (fig. 2 *a*).

With a suitable position of the electrodes, consequently, the flame is shown to be not polarized lengthwise at all. This proves that the longitudinal polarization of the flame is only apparent, called forth by the unequal immersion of the two electrodes. At the same time the second point, the constancy of the value of the potential in the flame, is hereby explained.

The reversal of the polarity can likewise be shown with a gas-flame; only it does not bring back the electrometer-needle quite to its position of rest, a small ± deflection of from 5 to 10 scale-divisions always continuing to subsist.

A bisulphide-of-carbon flame shows the reversal of the polarity in like manner as a spirit-flame, which is interesting inasmuch as in it the chemical process is fundamentally different.

Leaving quite out of consideration provisionally a proper electricity of flame, the electrical phenomena in question might be essentially conditioned by contact of the metals with the hot air and the gases of the flame. It might then be expected that the electromotive force would depend on the nature of the metals, as well as on that of the burning gases.

A series of very carefully made experiments have most decisively confirmed both these conclusions.

§ 6. *Dependence of the Electromotive Force upon the Nature of the Metals.*

If a platinum electrode is brought into the base of the flame or into the sensitive stratum of air while the apex is, as exactly as possible, at the same place conducted to earth, considerably different values are obtained, according to the nature of the conducting metal. In the series of experiments recorded in the following table the position of the electrodes was that represented in fig. 2 *a*. B, as well as the flame itself, remained unmoved during the whole time of a series of experiments, while the apex-electrode S consisted successively of wires of platinum, iron, copper, and aluminium. The experiments were made with the non-luminous flame of the small Bunsen burner described at the commencement.

Table I.

The flame-apex electricity conducted away by	Electromotive force for D=100. Series of experiments. I.	II.	III.	IV.
Platinum . .	49·6	116·0	157·2	188·4
Iron	64·3	139·0	173·8	232·0
Copper . . .	...	153·2	208·8	264·0
Aluminium . .	171·0	237·0	268·8	364·0

In all four series the flame shows itself very highly electric when conducted away by aluminium, less so with conduction by copper, still more feebly on the employment of iron; and the smallest values are obtained with conduction by platinum.

When both electrodes dip into the flame the result is completely analogous; and it is just the same when, instead of the non-luminous gas-flame, a luminous gas-flame or a spirit-flame is employed. The peculiar position of aluminium also with such an arrangement of the electrodes and the employment of such flames follows from Table II.

Table II.

Kind of flame.	Apex of flame conducted-from by Platinum.	Aluminium.	Electrodes as in
Ordinary Bunsen burner	74·2	149·2	Fig. 2 *b*
Luminous gas-flame . .	49·6	112·2	Fig. 2 *b*
Spirit-flame	160·0	278·0	Fig. 2 *a*

The base-electrode, in all the series of experiments, consisted of a platinum wire. As the wires employed were not of equal thickness, we made another series of experiments with three plates, of exactly equal thickness, of aluminium, copper, and

platinum, which were introduced into the apex of a pure-alcohol flame. In the gas-flame there was always a fusion of the aluminium, altering the shape of the electrode. The possible source of error herein contained also disappears when a spirit-flame is employed. The determinations of the electromotive force of the flame were:—when its apex was conducted-from by

The platinum plate	120·1
The copper plate	166·0
The aluminium plate	301·5

Sodium and magnesium are more negative than aluminium, as will be seen from the following numbers:—

Flame conducted-from by	Electromotive force.
Platinum wire	119
Aluminium wire	198
Magnesium ribbon	221
Sodium	338

The last two metals were introduced into the lower part of the flame, in order to prevent their ignition. The sodium was a piece of the size of a bean, with a bright cut surface.

The relative position to one another of all the metals employed is specified by the following numbers:—

Purchased wires of	Gold (not pure), Platinum, Silver (not pure)	150
	Iron	170
	Copper	200
	Aluminium	300
	Magnesium	320
	Sodium	500
	A Daniell's element	=100

It is consequently put beyond question that the electric condition of a flame depends essentially upon the nature of the metal conducting from it; but that nevertheless the quality of the surface of the electrode which is in air plays also an essential part was evidenced by the following experiments:—

If the insulated base-electrode in air be wetted with water or a salt-solution while the apex of the flame communicates with the earth, a very considerable diminution of the electromotive force immediately takes place, especially when a solution of potassium chloride is employed.

A perfectly clean platinum wire, employed as the base-electrode, gave $E=184$; when it was wetted with distilled water, E instantly fell to 134, passed through the values 148,

160, and was finally constant at 170. That the former value 184 was not again reached after the evaporation of the water may probably be accounted for by a slight impurity of the (commercial) distilled water.

Still more striking was the phenomenon when a solution of potassium chloride was employed. Here likewise an instantaneous diminution of the electromotive force from 184 to 74 took place; and when the wire was once hastily drawn through the flame so that small particles of fused potassium chloride overspread it, the electromotive force fell quite to 16. This value could not be increased by any shifting of the electrode; so that the cause of this great diminution cannot possibly be the unavoidable change of place of the electrode concerned.

Finally, let us mention one more circumstance belonging to this—namely that freshly annealed wires, used as electrodes in the air stratum, always give higher values than those which have remained a longer time (say ten minutes) exposed to the air—the explanation of which behaviour, even in the case of platinum, can only be found in an alteration of the quality of the surface.

§ 7. *Repetition of the Experiments with Liquid Electrodes.*

In order to completely avoid the contact of the flame-gases with metals, liquid electrodes, of the form represented in fig. 3, were employed. By the pressure of the liquid column in the glass tube R a drop was pressed out of the fine aperture *a*, which was then brought into the air surrounding the flame, and as near as possible to its base. A U tube served to put the flame of a Bunsen burner constructed entirely of glass into communication with the earth, one leg of which ascended the inner cavity of the burner. Both the glass electrodes were filled with distilled water, into which clean platinum wires (Pt) dipped. When the two water columns in the electrodes were connected directly with each other, the electromotive force called forth by the heterogeneity of the platinum electrodes amounted, at the maximum, to 0·05 daniell.

Now, in all the experiments, the electrode which was in air was charged in the same sense as a metallic electrode; it likewise was positive, though the electromotive force was certainly much weaker. The mean values from three series of experiments were the following:—

$$E=51, \quad E=51, \quad E=56,$$

while two platinum electrodes at the same flame gave values which lay between 150 and 180.

A similar series of experiments were performed with a

spirit-flame, in which direct contact of any metal with the flame was likewise avoided. The conduction to earth took place by means of a platinum plate dipping into the alcohol of the lamp, and connected with the earth-conduction by a platinum wire. In addition to the electrode represented in fig. 3, a wet string wrapped tightly round a glass rod was employed. The electromotive force was then determined:—For the

Water electrode in air . $E=24$,
Wet string in air . . . $E=58$;
For a platinum electrode $E=99$.

That lower values likewise result for the latter than in the previous experiments cannot be surprising, since in this arrangement of the experiment there is no *second* metal dipping into the flame itself.

A direct determination of the combination platinum, water, alcohol, platinum gave a maximum of 11·6 for a daniell$=100$; so that the observed electromotive force cannot be produced by this.

In employing the wet string, care must be taken not to place it tangentially near the flame; for then small fibrils might project into the flame itself, by which, for the reasons above discussed, a reversal of the polarity of the flame might easily be induced.

The above-communicated values being so much lower than on the employment of platinum electrodes may be accounted for by the conduction to earth by distilled water or alcohol being always very imperfect. On this account it seemed advisable to examine the behaviour of a water electrode over against a platinum electrode. If the apex of the flame is conducted-from to earth by a platinum wire, the water electrode which is in air is positive only so long as the platinum wire is completely enveloped by the flame; if it be drawn so far out that it also is entirely in the hot-air stratum, the polarity of the electrodes is reversed—the platinum wire being positive, the water electrode negative. For example, in an experiment of this kind, by the drawing-back the electromotive force was raised from -142 to $+60$ (the signs refer to the metal electrode). From this it follows that metals in contact with hot air become more strongly positive than liquids, but that liquids in contact with heated gases exhibit nevertheless a similar behaviour to that of metals. Accordingly we must not directly infer, from the fact that flames show themselves electric even when all metals are avoided, the existence of a peculiar electricity of flame.

The complete analogy of behaviour between metal and water

electrodes appears also from the kind of metal which conducts from the flame to the earth having an essential influence upon the result. When the base-electrode was formed by water, and the electricity conducted from the apex of the flame by different metal plates of equal thickness, determination of the electromotive force E, when the conductor was

A platinum plate, gave $E = 73{\cdot}6$,
An aluminium plate, gave $E = 176{\cdot}8$.

The electromotive force of the combination aluminium, flame, hot air, water, platinum is therefore as much again as that between platinum, flame, hot air, water, platinum, completely in accordance with the previous experiments with two metal electrodes.

§ 8. *Dependence of the Electromotive Force on the Nature of the Flame.*

Since the electromotive force of flame depends on the nature of the metal introduced into it, it is to be expected that, when the constituents of the flame-gases are changed, an alteration of the electromotive force must also occur. Such an alteration can be readily brought about by introducing, for example, a bead of soda into the flame, on a well insulated wire. Indeed a deflection of the electrometer-needle then takes place immediately; only with sodium there is the great inconvenience that within a short time the entire atmosphere of the room is so impregnated with sodium vapour that the flame burns with a strong resemblance to a sodium-flame, which affects the trustworthiness of the results. On this account a salt to which flame is less sensitive was chosen, namely potassium chloride.

It was first ascertained, by a series of careful experiments, that the introduction of a well-cleaned and insulated platinum wire into the flame did not alter the electromotive force. It may be sufficient to allude to this point here, as we shall subsequently return to it.

When the electrodes are in the position shown in fig. 2 *a*, and a bead of potassium chloride is introduced on an insulated platinum wire, the needle receives an impulse which indicates an increase of the electromotive force; but it quickly goes back again, and, indeed, far below the value of the deflection which had been given with a pure flame. According to this, there was a diminution of the electromotive force; but it was only an apparent diminution; for if the bead of potassium chloride be now taken out of the flame, the needle approaches still nearer to its resting-place. This indicates that with the electrode which was in air an alteration must have taken place. Upon it a thin dash of potassium chloride has formed,

so that the platinum wire, introduced into the flame, burns for a moment like potassium. As soon as the colouring of the flame is over, the same wire, used as the electrode in air, again gives the usual (mostly somewhat higher) values.

The apparent diminution of the electromotive force is consequently caused by the coating of the electrode which is in air with potassium chloride, corresponding to the experiment recorded in § 6.

In the following Table, E denotes the electromotive force of an absolutely pure non-luminous gas-flame; E_k, the electromotive force of the same flame when a bead of potassium chloride is introduced; E', the electromotive force of the flame after removal of the bead, but with the electrodes B and S (fig. 2 *a* & *e*) covered with Ka Cl. Accordingly $E_k - E'$ represents the increase of electromotive force produced by the introduction of the Ka Cl.

Series.	E.	E .	E'.	$E_k - E'$.	Position of the electrodes as in
I.	150	60	26	34	Fig. 2 *a*.
II.	171	111	80	31	
III.	182	142	30	112	Fig. 2 *e*.
IV.	174	132	75	57	

The reason that the values of $E_k - E'$ show so little accordance lies in the impossibility of making two series of experiments under exactly the same conditions. Besides depending on the position of the electrodes, $E_k - E'$ depends, in a more complicated manner, on this—into which part of the flame the potassium-chloride bead is introduced. Nevertheless the above numbers prove that an increase of the electromotive force is produced by the vaporization of the potassium chloride in the flame. This can also be verified on the employment of liquid electrodes.

Different flames being employed, the following values were obtained for the electromotive force when platinum electrodes were introduced in the position fig. 2 *a*:—

Flame.	E.
(1) That of a Bunsen burner . .	180–200
(2) A luminous gas-flame . . .	180–200
(3) Stearine candle-flame	180–200
(4) Spirit-flame	180–200
(5) Magnesium-flame	20– 30
(6) Bisulphide-of-carbon flame . .	85–100

With magnesium the experimental difficulties are very great. Here it could not be ascertained with certainty whether the air-electrode was excited positively, *as was at other times the case with all flames*, or negatively.

§ 9. *Combination of several Flames.*

We have still to mention that flames can be combined in exactly the same manner as galvanic cells—the base of one flame being connected with the apex of the second, the base of the second with the apex of the third, and so on, by wires. Three Bunsen burners, connected in this manner by copper wires, gave the following deflections at the electrometer:—

1 burner . . .	80 scale-divisions.		
2 burners . . .	156	„	(160)
3 „ . . .	245	„	(240)

With perfect equality of the burners the numbers in brackets might have been expected. Twenty-five spirit-flames, combined in this way into a battery, produced at the electrometer a deflection too small to be measured by mirror and scale. At all events the intensity of the current which set in was, on account of the great resistance within the flames, very little. A sufficiently sensitive multiplier to prove the latter point was not at our disposal.

§ 10. *Summary of the Results.*

1. The longitudinal polarization of flame is only apparent, and is called forth by unequal immersion in the flame of the wires employed as electrodes.

2. The flame appears to be strongly polarized in its cross section; and the electrode which is in the stratum of air immediately enveloping the flame is always positive to the electrode in the flame.

3. In agreement with the points 1 and 2, the electromotive force is independent of the magnitude of the flame.

4. Change of polarity of the flame can be called forth by a suitable displacement of the electrodes, and likewise finds its explanation in points 1 and 2.

5. The electromotive force of the flame is dependent on the nature of the metals used as electrodes, and on that of the burning gases. It appears singularly powerfully electric when aluminium or magnesium is made use of as the metal conducting from it; singularly feebly when the electrode in air is covered with a salt (potassium chloride).

6. Unequivocal electrical effects are likewise obtained from the flame when water electrodes are employed and every metal excluded; and the electrode which is in air is likewise positive to that in the flame. All the above propositions can be confirmed with liquids, so far as their nature permits.

7. Flames can be combined after the manner of galvanic

elements; consequently a number of them can be united to form a "flame battery."

§ 11. *Thermoelectrical Behaviour of Platinum Wires separated by a Stratum of Hot Air*.*

The phenomena above discussed can be naturally explained both from the thermoelectric and the electric theory; but we soon arrived at the conviction that, so long as experiment was made on the flame itself, a decisive experiment for the one or the other theory could not be instituted, especially if the existence of a peculiar flame-electricity (hitherto excluded from our considerations) were assumed in order to aid in explaining the phenomena. Hence it was necessary to consider the matter from another point of view, and to discover a method by aid of which wires at different temperatures in hot air could be examined as to their respective electrical behaviour. Of course in this case the hot air would not proceed from a flame, and therefore would not be mixed with the products of combustion.

Starting from these views, we employed the apparatus represented in fig. 4. $a\,b$ is a fine platinum wire stretched between two copper wires x and y, which can be rendered incandescent by a battery of two Bunsen elements B. At the point u, its electricity, and with it also that of the battery, was conducted to earth and connected with one pair of quadrants of the electrometer. A second platinum wire, c, was connected with the insulated pair of quadrants, and could be brought to any degree of proximity to the wire $a\,b$. This movable wire was placed so as to be as near as possible to the point u; if a thermoelectric difference then arose from the incandescence of the wire $a\,b$, it was necessarily announced by the electrometer.

There is, however, in this experiment a source of error to be mentioned. As it would be inadmissible, and even (with precision) impossible, to place the wire c exactly opposite to the point u, a difference of potential might also possibly arise from the circumstance that the potential-difference of the points u and v on the stretched wire $a\,b$ traversed by the current would be measured through the intervention of the conductivity of the heated air. In order to be independent of this, a turn-plate W_1 was inserted in the circuit, by which the direction of the current in the wire $a\,b$ could be altered. If

* The electric excitation here occurring is taken into consideration in this place *only so far* as it is *immediately* connected with the electricity of flame. The general treatment of this phenomenon is reserved for a future communication.

with one position of the turn-plate the value of the potential is $+x$, it will be converted into $-x$ by rotating the turn-plate; that is, the direction of the electrometer-deflection must be right or left according to the position of the turn-plate W_1.

The circumstance here discussed, indeed, rendered a small correction necessary. Assuming that the wire c would behave like a platinum electrode introduced into the hot stratum of air of a flame, we can denote the value of the potential upon it by $+e$. To this value $+e$ the value of the potential at the point v will be added or subtracted from it, according to the position of the turn-plate W_1. If we denote by s_1 and s_2 the deflections of the electrometer corresponding to the two positions of the turn-plate, we have

$$e+x=s_1, \quad e-x=s_2,$$

consequently

$$e=\frac{s_1+s_2}{2}.$$

If the earth-conduction is brought, not to the point u, but, say, to the point r, this method still remains applicable; only now x frequently $\geqq e$, which for the moment slightly disturbs the clearness of the experiment.

The result was now obtained that a platinum wire c, brought near to the incandescent wire, received a powerful positive charge, consequently behaved like the base-electrode of the flame. The agreement goes so far that even the values of the electromotive force lie within the same limits. Nay, the analogy between the two phenomena is still closer; for nearly all the experiments above given for the flame can be repeated with such an incandescent wire.

The electromotive force, besides depending on the distance of the wire c from the wire $a\,b$, turns out to be dependent on:—1, the state of incandescence of the wire $a\,b$; 2, the quality of the surface of the wire c.

The correctness of these propositions follows from the following Table, in which the numbers are, for clearness, reduced to equal sensitiveness of the electrometer. (A double deflection of the daniell = 100.)

Table III.

Series 1 ($x \geqq e$). Conduction to earth in the point r (fig. 4).

$x+e$.	$x-e$.	e.	Position of the wire c.
+151·0	−10·0	161·0	½ millim. above $a\,b$ (fig. 4).
+150·6	−16·1	166·7	
+169·5	+ 6·0	163·5	
+155·0	− 3·0	158·0	½ millim. laterally from $a\,b$.
+163·9	+ 0·8	163·1	

When the wire c was brought half a millimetre below the wire $a\,b$, there was no constant orientation of the electrometer-needle. The following are the maximum values obtained:—

$x+e$.	$x-e$.	e.
129	−9	138

Series 2 ($e>x$). Conduction to earth in the point u. Dependence on the state of incandescence of the wire $a\,b$.

Incandescence of the wire $a\,b$.	$e+x$.	$e-x$.	e.
Dull red	54	42	96
Bright red	86	83	169
White	74	42	116

The last series of experiments show that, with the same position of the wire c ($\frac{1}{2}$ millim. above $a\,b$), the value of e is lower at a white heat than at a bright red heat—a very surprising circumstance, but confirmed by many control experiments.

If the wire c be covered with a coat of potassium chloride, the electromotive force sinks considerably; in one experiment it fell from 188 to 34.

The wire c was now replaced by a water electrode (fig. 3) or a wet string. The experiments showed indubitably that liquids also, separated from glowing platinum by a stratum of hot air, become electrically excited; only this excitation, exactly as with the flame, is much less. The results were, with

	$e+x$.	$e-x$.	e.
c a platinum electrode . .	85	84	169
c a water electrode . . .	26	15	41
c a wet string	26	24	50

Accordingly, from the fact that flames show themselves distinctly electric even when all contact with metals is avoided, it must not at once be concluded that they have an electricity peculiar to them.

Let it be further remarked that, both when the wet string and when the water electrode was employed, the amount of the potential-difference between $a\,b$ and the wire in contact with the liquid of the electrodes, when directly connected by water, was determined before the definitive experiments. The deflection of the electrometer-needle amounted for it to only a few divisions of the scale; so that no source of error could spring from this.

The phenomena here discussed, which had not, to our knowledge, been before observed, stand evidently in the closest con-

nexion with an experiment described by Edlund *—namely, that when the incandescent carbon points of the electric lamp are connected by a multiplier immediately after the extinction of the flame-arc, a strong thermoelectric current is indicated. It follows also from the above-communicated experiments that if the carbon points in the flame-arc are in different degrees of ignition (it is well known that the positive is the hotter; this condition is therefore fulfilled), an electromotive counterforce must arise, the quantity of which essentially depends on the nature of the conductors between which the flame-arc passes—a deduction which has already been verified by Edlund.

The positive electrode, as the hotter, must behave like the incandescent wire in our experiment—that is, be thermoelectrically negatively excited,—which indicates the rise of an electromotive counterforce.

§ 12. *Dependence of the Electricity of Flames on the State of Incandescence of the Electrodes.*

Having thus shown that platinum wires, as well as water electrodes, in contact with hot air are electrically excited, we return to the electricity of flame.

The method discussed in the preceding section permits also the determination of the electromotive force of the element "incandescent platinum, hot air, flame-gases, incandescent platinum," if the wire *a b* is introduced into the stratum of hot air, and the wire *c* into the apex of the flame.

As long as the wire *a b* (fig. 4) does not glow, it is positive to the wire *c* in the flame; but as soon as it becomes incandescent a negative value is added to the positive value of the potential; therefore the potential-difference between the two electrodes must undergo a diminution. This inference was completely verified by experiment.

In the following, E denotes the electromotive force of the flame when *c* is incandescent and the wire *a b* not, and *e* the electromotive force of the flame when both wires are incandescent. Of necessity *e* would be =E if the incandescence of the wire *a b* had no influence; yet there resulted :—

Series I.		Series II.	
E.	*e*.	E.	*e*.
254	148	134	47
246	147	122	50
216	116	114	37

* Pogg. *Ann.* cxxxi. p. 586 (1850), & cxxxiii. p. 353 (1851).

consequently always a considerable diminution of the electromotive force.

The difference of the numbers in one and the same column arises from the circumstance that for each new determination the wire *a b* was brought into a somewhat different place at the margin of the flame.

An experiment which likewise proves the dependence of the electromotive force on the state of incandescence of the electrodes, but which is not so free from objection as the above, is the following:—The electrode S (fig. 2 *a*) was replaced by a platinum pan, and the electromotive force E determined. Water was then introduced into the red-hot pan, and, as soon as it boiled, the quantity E measured again. When the whole of it was evaporated and the pan again red-hot, the first experiment was repeated for a control. In this the turn-plate W (fig. 1) was placed so that the platinum pan was conducted-from to earth. A long series of experiments constantly gave the same result, namely a considerable diminution of the electromotive force with diminution of the temperature of the pan. For example,

(1)	With the pan	red-hot	. . . .	E=216
	,, ,,	at 100°	. . . .	E=152
	,, ,,	red-hot (control-experiment)	.	E=213
(2)	,, ,,	red-hot	. . . .	E=196
	,, ,,	at 100°	. . . .	E=118
	,, ,,	red-hot (control-experiment)	.	E=197

These experiments, without the confirmation afforded by the preceding experiment, did not appear to us definitive, because an alteration of E might possibly be effected by the evaporation of the water and by the wetting of the outside of the pan. But from the former experiment it was already evident that the electromotive force is the greater the greater the difference of temperature between the electrode in the flame and that in the air.

§ 13. *Thermoelectrical Behaviour of Wires within a Flame.*

In contradiction to the fundamental experiments adduced in the last section stands the fact that, in spite of great differences of temperature, no thermoelectric excitation takes place when *both* electrodes dip equally into the flame. With the electrodes arranged as represented in fig. 2 *c* it is easy to place the electrode B so that it does not glow while S is intensely white-hot; but in spite of this the electrical forces which arise are very slight, as the following experiment shows:—

Electrode S (fig. 2 *c*) white-hot. Base-electrode B

In the flame (fig. 2 *c*).		In air (fig. 2 *a*).
Dark . . .	$E' = +4$	
Red-hot . .	$E' = +3$	$E = +150$
White-hot . .	$E' = -6$	

The sign prefixed refers to electrode B.

Also when the electrodes are in one cross section of the flame and at the same time dip completely into it, in spite of great differences of temperature the electromotive force is very small. If the wires *a b* and *c* (fig. 4) were brought into the coolest (lowest) part of an alcohol-flame, E was ascertained to be 23. By a suitable shifting of the wire *c* this could be reduced to 6, notwithstanding that *c* was not red-hot while *a b* was put into a dazzling white incandescence by an electric current. The reason for this surprising behaviour appears to be that the flame-gases, being relatively good conductors in comparison with the hot air, prevent the electrical difference from being completed. This would also be confirmed by the fact that, in the experiments with wires in air, the maximum is found when the wire *a b* is bright red-hot. White-hot wires, when the electrodes are in the same position, constantly give lower values for the electromotive force, as we have already mentioned above—which, we think, can only be accounted for by the conductivity of the surrounding air being so augmented by the strong heating that it forms as it were a secondary closing of the circuit.

§ 14. *Cases in which the Thermoelectric Excitation predominates over the Electrolytic, and* vice versâ.

The view last discussed explains also very naturally the experiment mentioned in § 4, that the electromotive force rises from 12 up to 190 as soon as the electrode B (fig. 2 *a*) is drawn quite out of the flame. The moment this takes place, the secondary closing formed by the flame is removed, and the thermoelectric force corresponding to the temperature-differences of the electrodes comes fully into action.

If this explanation be not admitted as the correct one, it may appear doubtful if *with a flame* the thermoelectric excitation does not entirely *fall away*, and the electrolytic exclusively condition the electric behaviour of the flame. But if the experiments related in § 12 tell against this, others also can be instituted which it would be difficult to explain without admitting a thermoelectric excitation. They are the following:—

If the apex of the flame be conducted-from to earth by a

platinum pan, and a fine platinum wire, dazzlingly white-hot, be brought below it into the extreme margin of the flame, the platinum wire shows free *positive* electricity. According to the previous experiments, however, a white-hot wire ought to appear *negatively* charged with respect to the red-hot pan. In this case, therefore, the electrolytic excitation outweighs the thermoelectric. But if the difference of temperature between the two electrodes be made still greater (which can easily be done by cooling the platinum pan with water), *the polarity of the flame is reversed,* the white-hot platinum wire is consequently now negative, as the thermoelectric theory requires it to be.

In an experiment of this sort the following values were obtained:—

$$\begin{array}{lll} (1) & \text{Pan red-hot} \quad . \quad . \quad . \quad . \quad . & (-) \\ & \text{Platinum wire white-hot} \quad . & (+) \end{array} \Bigg\} E = +20$$

$$\begin{array}{lll} (2) & \text{Pan cooled by } H_2O \quad . \quad . & (+) \\ & \text{Platinum wire as above} \quad . & (-) \end{array} \Bigg\} E = -32.$$

The values of E are considerably lower, because in this experiment *both* electrodes are in the flame. At the same time the margin appears *negative* to the interior of the flame—a behaviour which cannot be observed under ordinary conditions.

It can, further, be shown that an intensely white-hot platinum wire *in air* is negative to one not red-hot *in the flame.* If the wire *a b* (fig. 4) was stretched at about 3 millim. distance from the margin of the flame, and *c* introduced into the foot of the flame so as to be completely enveloped by the flame-gases but at the same time not to become red-hot, then likewise the polarity of the flame was reversed as soon as *a b* was rendered brilliantly incandescent by the current. With the wire *a b* dark the electromotive force amounted to about 1 daniell; with it white-hot, to about 0·17–0·2 daniell, but with the deflection in the opposite direction to the former: therefore in this case the thermoelectric again outweighs the electrolytic excitation.

From these and all the preceding experiments it follows that we cannot explain the electric behaviour of flame by assuming either an exclusively thermoelectric or an exclusively electrolytic excitation, but that we must perforce regard *both* as cooperating in producing the total electrical state of the flame.

15. *On the proper Electricity of the Flame.*

If we imagine two platinum wires introduced into a flame and the stratum of air which envelopes it, there arises, accord-

ing to the above conception, a thermoelectrico-electrolytic element composed of

Cold platinum | Hot air + Hot air | Flame-gases + Flame-gases | Red-hot platinum.

If we fix our attention upon the single members of this combination, it is proved by the experiments that an electric excitation takes place between cold platinum with hot air, on the one hand, and incandescent platinum with flame-gases, on the other; while the question is still undecided whether an electrical difference exists between the hot air and the flame-gases even without wires or liquids being in contact with those gases. This question is identical with that whether a proper electricity does or does not belong to flame.

In order to bring this point also to a decision, let us here adduce a few more experiments, *which decidedly speak against the existence of a proper electricity of flame.*

On the hypothesis that to the stratum of air A A′ (fig. 1) enveloping the flame a certain quantum of positive electricity is brought by the process of combustion or by mere contact, at least a partial equalization of the electricities must take place, even if we take into consideration the bad conductivity of the two strata of gas, as soon as one or more well-insulated wires are passed transversely through the flame. But the potential-difference existing between the electrodes S and B (fig. 1) is not at all or only very slightly altered thereby. Let E be the electromotive force without the transverse wire, and E_D the electromotive force with it (platinum).

One series of experiments gave

$$E = 169{\cdot}0 \quad 164{\cdot}8 \quad 168{\cdot}4 \quad \text{mean } 167{\cdot}4,$$
$$E_D = 162{\cdot}0 \quad 161{\cdot}2 \quad \ldots\ldots \quad \text{mean } 161{\cdot}6,$$

consequently a diminution of about 3 per cent.

A second series, when two very carefully cleaned and well-insulated platinum wires were passed transversely through the flame, gave:—

	E.	E_D.	$E-E_D$.
Experiment I.	168·5	168·0	+0·5
II.	158·4	158·9	−0·5
III.	182·0	182·0	0·0

Each of the above numbers is the mean of five readings; and the position of the electrodes was somewhat altered from one experiment to another; hence the difference in the numbers in the same column. Therefore a partial equalization of

the electricity does not take place through the introduction of the transverse wires.

Further, in contradiction to the existence of a proper electricity of flame is the fact that when platinum wires as homogeneous as possible are used as electrodes, and are also approximately in the same state of incandescence, the electromotive force of the flame sinks to a minimum. The electromotive force of the combination

White-hot platinum | Hot air + Hot air | Flame-gases + Flame-gases | White-hot platinum

was ascertained to be 0·0013 daniell—a value which lies within the limits of errors of observation, and consequently may be put = 0. In this determination, of course, it was necessary to employ the method discussed in § 11.

For e the following values were obtained:—

$e+x$.	$e-x$.	e.	
+9·5	−9·0	+0·5	Daniell = 100;
−1·7	−0·2	−1·9	
+7·3	−5·5	+1·8	

consequently $e = 0{\cdot}0013$ daniell. According to this, the electromotive force of the member

Hot air | Flame-gases

may be put = 0, and consequently a proper electricity of the flame be left out of consideration.

The most important circumstance contradicting the existence of a proper electricity of flame may be that the reversal of the polarity of the flame is not connected with the reversal of the combustion-process.

A flame of air burning in an atmosphere of illuminating-gas exhibits the same polarity as illuminating-gas burning in air.

An incandescent platinum wire in burning air was strongly negative to the metal (copper) out of which the flame issued; and a second wire, introduced into its sensitive stratum, received a strong positive charge, just like the base-electrode of an ordinary flame. The details of this experiment were as follows:—

First the electromotive force of an ordinary gas-flame issuing from a glass tube provided with a copper jet, with the electrodes in a determined position (fig. 2 *a*), was measured. A current of air was then passed through the tube, which was placed in a space filled with illuminating-gas, and the air-current ignited by the spark of an induction-apparatus. No source of error was given rise to by this (as we convinced

ourselves by numerous preliminary experiments), since the two electrodes were metallically connected with one another and with the earth before the flame was kindled. As the shape of the small bluish flame was quite different from that of a gas-flame in air, it was necessary to shift the base-electrode somewhat, in order to attain an analogous position to that in the first experiment.

For the electromotive force of the

Gas-flame in air, we found $E=148$
Air-flame in gas, „ $E=152$

In both cases the glowing wire was negative, the not glowing one positive, consequently the apparent polarity of the flame the same.

Lastly, another noteworthy circumstance should be mentioned: namely, the polarity of the air-flame appears reversed when the flame-electrode is in the lowest and, therefore, coolest part and is not red-hot. We have seen, in § 13, that, with an analogous position of the electrodes in an ordinary flame, the dark wire in the flame was always negative to one in hot air. There we had the combination

Platinum, *Hot gas*, *Hot air*, Platinum.

But with the air-flame we have

Platinum, *Hot air*, *Hot gas*, Platinum,

consequently the same elements in inverse order, from which the reversal of the polarity of the flame results spontaneously.

§ 16. *Theory and Conclusions.*

On the basis of the above experiments the following theory on the electricity of flame can be set up.

By the process of combustion in itself free electricity within the flame is not generated; on the other hand, the flame-gases and the air stratum immediately enveloping the flame possess the property, when in contact with metals or liquids, of exciting it similarly to an electrolyte. To this electrolytic excitation is added a thermoelectric excitation, produced by the state of incandescence of the electrodes. The quantity and kind of the electric excitation is then

(1) Independent of the size of the flame;

(2) Dependent on the nature, and the quality of the surface, of the electrodes;

(3) Dependent on the nature of the combustion-gases;

(4) Dependent on the state of incandescence of the electrodes.

These conclusions are confirmed by numerous experiments; and no experiment has been found to contradict them.

The decision in favour of this theory was supplied by the fact that glowing and cold wires separated only by heated air, with the exclusion of combustion-gases, showed an electrical difference. Here also the latter is dependent on the nature, and the quality of the surface, of the electrodes employed, and on their state of incandescence. A too strong heating of the wires, and, therewith, also of the separating air stratum, proved unfavourable to the development of free electrical tension—a circumstance probably due to the augmentation of the conducting-power of that separating stratum. In accordance with this, wires introduced into the flame, so long as they are both immersed in the combustion-gases (which are relatively good conductors), never give the maximum of potential-difference; rather this enters only when one of the wires comes into contact with only the outer air stratum of the flame (which is endowed with a very high resistance).

The occurrence of a thermoelectric counterforce within the galvanic flame-arc is also naturally explained by the above theory.

The questions proposed at the commencement are therefore to be answered thus:—Hankel's theory is not in accordance with experiment; and the two kinds of excitation assumed by Buff and Matteucci must be regarded as *simultaneously* causing the apparent electricity of flame.

Wolfenbüttel, February 1882.

XX. *On the Equilibrium of Liquid Conducting Masses charged with Electricity.* *By* Lord Rayleigh, *F.R.S.**

In consequence of electrical repulsion, a charged spherical mass of liquid, unacted upon by other forces, is in a condition of unstable equilibrium. If a_0 be the radius of the sphere, Q the charge of electricity, the original potential is given by

$$V=\frac{Q}{a_0}.$$

If, however, the mass be slightly deformed, so that the polar equation of its surface, expressed by Laplace's series, becomes

$$r=a(1+F_1+F_2+\ldots+F^n+\ldots),$$

* Communicated by the Author.

then

$$V=\frac{Q}{a_0}\left\{1-\Sigma(n-1)\iint\frac{F_n^2 d\sigma}{4\pi}\right\};$$

and the potential energy of the system reckoned from the equilibrium position is

$$P'=-\frac{Q^2}{8\pi a_0}\Sigma(n-1)\iint F_n^2 d\sigma.$$

In actual liquids this instability, indicated by the negative value of P', is opposed by stability due to the capillary force. If T be the cohesive tension, the potential energy of cohesion is given by

$$P=\tfrac{1}{2}a_0^2 T\,\Sigma(n-1)(n+2)\iint F_n^2 d\sigma\,*.$$

If $F_n \propto \cos(pt+\epsilon)$, we have for the motion under the operation of both set of forces,

$$p^2=\frac{n(n-1)}{\rho a_0^3}\left\{(n+2)T-\frac{1}{4\pi}\frac{Q^2}{a_0^3}\right\}.$$

If $T>\dfrac{Q^2}{16\pi a_0^3}$, the spherical form is stable for all displacements. When Q is great, the spherical form is unstable for all values of n below a certain limit, the maximum instability corresponding to a great, but still finite, value of n. Under these circumstances the liquid is thrown out in fine jets, whose fineness, however, has a limit.

The case of a cylinder, subject to displacement in two dimensions only, may be treated in like manner.

The equation of the contour being in Fourier's series

$$r=a(1+F_1+\ldots+F_n+\ldots),$$

we find as the expression for the potential energy of unit length

$$P'=-\frac{Q^2}{l^2}\Sigma(n-1)\int\frac{F_n^2 d\theta}{2\pi},$$

Q being the quantity of electricity resident on length l.

The potential energy due to capillarity is

$$P=\tfrac{1}{2}\pi a T\,\Sigma(n^2-1)\int\frac{F_n^2 d\theta}{2\pi},$$

and for the vibration of type n under the operation of both

* See Proc. Roy. Soc. May 15, 1879.

sets of forces,

$$p^2 = \frac{n^2-n}{\rho a^3}\left\{(n+1)\mathrm{T} - \frac{2\mathrm{Q}^2}{\pi l^2 a}\right\}.$$

The influence of electrical charge in diminishing the stability of a cylinder for transverse disturbances may be readily illustrated by causing a jet of water from an elliptical aperture to pass along the axis of an insulated inductor-tube, which is placed in connexion with an electrical machine. The jet is marked with a recurrent pattern, fixed in space, whose wave-length represents the distance travelled by the water in the time of one vibration of type $n=2$. When the machine is worked, the pattern is thrust outwards along the jet, indicating a prolongation of the time of transverse vibration. The inductor should be placed no further from the nozzle than is necessary to prevent the passage of sparks, and must be short enough to allow the issue of the jet before its resolution into drops.

The value of T being known (81 C.G.S.), we may calculate what electrification is necessary to render a small raindrop of, say, 1 millimetre diameter unstable. The potential, expressed in electrostatic measure, is given by

$$\mathrm{V} = \frac{\mathrm{Q}}{a_0} = \sqrt{(16\pi a_0 \mathrm{T})} = 20.$$

The electromotive force of a Daniell cell is about ·004; so that an electrification of about 5000 cells would cause the division of the drop in question.

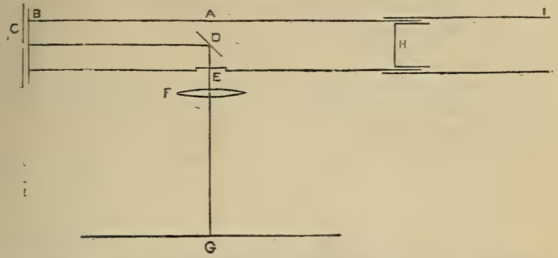
XXI. *On an Instrument capable of Measuring the Intensity of Aerial Vibrations.* *By* Lord Rayleigh, *F.R.S.**

THIS instrument arose out of an experiment described in the 'Proceedings of the Cambridge Philosophical Society'†, Nov. 1880, from which it appeared that a light disk, capable of rotation about a vertical diameter, tends with some decision to set itself at right angles to the direction of alternating aerial currents. In fig. 1, A is a brass tube closed at one end with a glass plate B, behind which is a slit C backed by a lamp. D is a light mirror with attached magnets, such as are used for reflecting-galvanometers, and is suspended by a silk fibre. The light from the slit is incident

* Communicated by the Author.

† See also Proc. Roy. Soc. May 5, 1881, p. 110.

upon the mirror at an angle of 45°, and, after reflection, escapes from the tube through a glass window at E. It then



falls upon a lens F, and throws an image of the slit upon a scale G. At a distance DH, equal to DC, the tube is closed by a diaphragm of tissue paper, beyond which it is acoustically prolonged by a sliding tube I.

When the instrument is exposed to sounds whose half wave-length is equal to C H, H becomes a node of the stationary vibrations, and the paper diaphragm offers but little impediment. Its office is to screen the suspended parts from accidental currents of air. At D there is a loop; and the mirror tends to set itself at right angles to the tube under the influence of the vibratory motion. This tendency is opposed by the magnetic forces; but the image upon the scale shifts its position through a distance proportional to the intensity of the action.

As in galvanometers, increased sensitiveness may be obtained by compensating the earth's magnetic force with an external magnet. Inasmuch, however, as the effect to be measured is not magnetic, it is better to obtain a small force of restitution by diminishing the moment of the suspended magnet, rather than by diminishing the intensity of the field in which it works. In this way the zero will be less liable to be affected by accidental magnetic disturbances.

So far as I have tested it hitherto, the performance of the instrument is satisfactory. What strikes one most in its use is the enormous disproportion that it reveals between sounds which, when heard consecutively, appear to be of the same order of magnitude.

June, 1882.

XXII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VI. *By* C. R. ALDER WRIGHT, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School**.

On the Relations between the Electromotive Forces of various kinds of Cells analogous to Daniell's Cell but differing therefrom in the nature of the Metals used, and the Chemical Affinities involved in the Action of these Cells.

I. *Cells containing Cadmium as one of the Metals, the Salts used being Sulphates.*

118. THE experiments described in Part V. (§§ 106–109) were repeated, using, instead of normal Daniell cells, analogous arrangements containing plates of cadmium, opposed in some instances to copper, in others to zinc, solutions of the respective sulphates being employed to surround the various plates used. With each of these two classes of cells (cadmium-copper and zinc-cadmium cells) the same result was obtained as that already recorded in the case of Daniell cells containing zinc- and copper-sulphate solutions—viz. that so long as the two solutions are of the same strength† the actual state of concentration of the fluids does not exert any appreciable influence on the E.M.F. generated with given plate-surfaces; at least the influence exerted is considerably less than the errors of observation and the variations due to unavoidable variations in the nature of the plate-surfaces, and does not amount to as much as $\pm$·0015 volt even

* Communicated by the Physical Society, having been read at the Meeting on June 24, 1882.

† It is convenient to define solutions "of the same strength" not as solutions of the kind usually spoken of by chemists as "equivalent" to one another, *i. e.* containing in a given volume quantities of dissolved matter in the ratio of the chemical equivalents of the substances dissolved (*e. g.* 159·5, 161, and 208 parts of anhydrous copper, zinc, and cadmium sulphate respectively), but as solutions in which *the dissolved matter and the water present are in the same molecular ratio*, i. e. which are expressible by parallel formulæ, such as $CuSO_4\,50H_2O$, $ZnSO_4\,50H_2O$, and $CdSO_4\,50H_2O$. With weak solutions the two definitions are practically the same—but not so with more concentrated fluids, especially when the molecular weights of the dissolved matters are considerably different (like $CuSO_4$ and $CdSO_4$). Solutions of zinc and copper sulphate of the same molecular strength are practically identical in specific gravity; but a solution of cadmium sulphate is considerably more dense than one of either zinc or copper sulphate of the same molecular strength. Thus solutions of the strengths $ZnSO_4\,50H_2O$, $CuSO_4\,50H_2O$, $CdSO_4\,50H_2O$ have at 18° specific gravities respectively close to 1·170, 1·167, and 1·208; with stronger solutions the excess of density of the cadmium solution is still more apparent.

when tolerably concentrated solutions of strength MSO_4 $50 H_2O$ are compared with similar solutions of only one twenty-fifth the strength, $MSO_4 1250 H_2O$.

On varying the nature of the surface of the cadmium plate (by employing bright cast metal, electro-deposited cadmium, or amalgamated cadmium), it was found that whatever result was produced in the cadmium-copper cells by a given alteration of the cadmium plate, every thing else remaining unaltered, *precisely the same numerical result, but with the opposite sign, was produced in the zinc-cadmium cells* by that alteration. Thus, substituting electro-cadmium for bright cadmium plates in the cadmium-copper cells caused an *increase* in the E.M.F. varying from ·002 to ·006 volt in numerous experiments, and averaging ·004 volt; whilst with the zinc-cadmium cells the same substitution caused a *decrease* in the E.M.F. varying between almost the same limits, ·002 and ·007 volt, and averaging almost the same value as before, viz. ·0045 volt. Similarly, on substituting amalgamated cadmium plates for bright cadmium in the cadmium-copper cells, the average effect was a *decrease* of ·0415 volt when the mercurial amalgam was fluid, and of ·015 volt when it had become solid and crystalline on standing; whilst with the zinc-cadmium cells, substitution for bright cadmium of amalgamated metal caused on an average an *increase* in E.M.F. of ·043 volt when the amalgam was fluid, and of ·016 volt when it had become solid and crystalline.

Cells containing Cadmium opposed to Copper.

119. On comparing together a number of similar pairs of cells containing in the one case electro-copper and in the other amalgamated copper, it was found that the average difference was sensibly the same as that observed when the same two kinds of copper plates were opposed to zinc (§ 107), viz. that, *cæteris paribus*, the cell containing amalgamated copper read on an average ·001 volt lower than the one containing freshly deposited electro-copper: the actually observed differences ranged from +·003 to −·003 volt, but were more usually negative.

As just stated, when the cadmium plate was amalgamated a decrease in E.M.F. was brought about, averaging ·0415 volt when the amalgam on the surface of the plate was fresh and perfectly fluid, and ·015 when perfectly solid and crystalline. Intermediate numbers were given by plates on the surface of which crystallization of the amalgam had begun but was not complete, the gradation being regular as the crystallization progressed.

The following table gives the average result, in volts*, of upwards of forty series of observations and comparisons, mostly extending over three to four hours, during which time the readings of each particular cell remained sensibly constant:—

Variation in E.M.F. due to the use of cadmium and copper sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4\ 47H_2O$, to $MSO_4\ 1250H_2O$)†	Less than ±·0015.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for electro-copper:—				
Amalgamated copper (surface wet with liquid mercury)	+·003	−·003	·006	−·001
Effect of substituting for bright cadmium:—				
Fresh electro-cadmium	+·006	+·002	·004	+·004
Amalgamated cadmium (surface wet with liquid mercury	−·050	−·033	·017	−·0415
Amalgamated cadmium (solid and crystalline)	−·020	−·005	·015	−·015
Electromotive force of combinations:—				
Electro-copper—Electro-cadmium	·756	·750	·006	·7525
„ „ Bright cadmium	·753	·745	·008	·7485
„ „ Amalgamated cadmium (liquid amalgam)	·717	·701	·016	·707
„ „ Amalgamated cadmium (solid amalgam)	·740	·727	·013	7335
Amalgamated copper—Electro-cadmium	·754	·749	·005	·7515
„ „ Bright cadmium	·752	·744	·008	·7475
„ „ Amalgamated cadmium (liquid amalgam)	·715	·701	·014	·706
„ „ Amalgamated cadmium (solid amalgam)	·737	·727	·010	·7325

When cells containing bright or electro-cadmium and electro-copper plates were allowed to stand for twelve hours or

* All the observations given in this paper are reduced to the same standard as that employed in Part V.—viz. the average reading at 15°·5 of a number of Clark's cells taken as 1·457 volt, the particular Clark's cells used being the same throughout.

† The specific gravities at about 19° of these fluids are close to the following:—

$MSO_4\ 47H_2O$ when M is cadmium: spec. grav. = 1·217.
do. when M is copper: spec. grav. = 1·175; solution nearly saturated.

$MSO_4\ 1250H_2O$: in each case below 1·01.

longer periods, a slight alteration in the E.M.F., due to formation of films of oxide on the surfaces of the plates, was usually noticeable. As with the normal Daniells (§ 108), the effect of the oxidation of the copper plate was to reduce the E.M.F. by a few thousandths of a volt; on the other hand, the formation of a film of oxide on the surface of the cadmium plate produced an *increase* in the E.M.F. of from ·001 to ·004 volt; so that in many cases the cell with partially oxidized plates gave sensibly the same value as a newly set-up cell, the diminishing effect of the oxidation of the copper being just about counterbalanced by the increasing effect due to the oxidation of the cadmium. In this respect cadmium behaves in the opposite way to zinc (§ 108).

Relations between the E.M.F. of Cadmium-Copper Cells and that corresponding to the net Chemical Action taking place therein.

120. According to Julius Thomsen's determinations (*Journ. prak. Chem.* ii. p. 233, and xi. p. 271), the heat of displacement of copper from copper-sulphate solution ($CuSO_4, 400H_2O$) by cadmium is as follows, in gramme-degrees per gramme-molecule:—

Cd, O, SO_3 aq. . . .	89,500
Cu, O, SO_3 aq. . . .	55,960
Difference =	33,540

the difference corresponding to 16,770 gramme-degrees per gramme equivalent, or ·740 volt*. As with normal Daniell cells (§ 114), a small quantity (x) is to be added to this, representing a variable correction dependent on the physical condition of the deposited copper. Evidently the average values above cited (·7475 to ·7525), obtained with bright and electro-cadmium, are sensibly the same as the value $·740+x$, thus deduced as representing the net chemical action taking place in the cell; *i. e.*, as with zinc-copper cells, the whole of the energy developed in the cell is adjuvant under the conditions obtaining in the above experiments.

In order to compare the results obtained with the amalgamated-cadmium cells with Julius Thomsen's figures, the heat of solution of cadmium (precipitated from the sulphate by zinc, crystalline) in twenty-five times its weight of mercury was determined by means of the calorimeter, 20 grams of

* The value 4410, used in the former parts of these researches for the factor for converting gramme-degrees into C.G.S. units, is employed throughout the present paper; *vide* § 103, footnote.

cadmium and 500 of pure mercury being employed for each experiment. To insure solution it was found necessary to wash the cadmium with dilute sulphuric acid just before use; otherwise portions remained unwetted and undissolved by the mercury. The final result arrived at as the average of several concordant observations was, that an evolution of heat to the extent of 610 gramme-degrees per gramme-molecule (112 grammes) of cadmium took place during solution. Hence, were cadmium sulphate formed from mercurial solution of metal instead of crystalline precipitated metal, the heat of formation expressed as Cd, O, SO_3 aq. would be $89{,}500-610=88{,}890$ (admitting that Thomsen's value 89,500 applies, without correction, to the metal in the crystalline condition of that experimented with). Consequently the heat of displacement of copper by cadmium from the sulphate is 32,930 per gramme-molecule, or 16,465 per gramme-equivalent, corresponding to ·726 volt. The observed values varied between ·701 and ·717, averaging ·707 with electro-copper and ·706 with amalgamated copper—again not differing from the value deduced from the thermal data by an amount materially outside the limits of experimental errors, especially those due to variation in the heat of formation of salts according as the physical state of the metal employed varies.

It is, however, to be noticed that the above heat of solution of crystalline cadmium in mercury only corresponds to an E.M.F. of ·0135 volt; whilst the average difference in E.M.F. caused by the substitution of fluid amalgamated cadmium for crystalline electro-metal was $·7525-·707=·0455$ volt, a considerably greater amount; so that amalgamating the crystalline metal appears to produce a greater effect on the E.M.F. than corresponds to the heat of solution. Just the same result is produced when cadmium and zinc are opposed (§ 121); on the other hand, the effect on the E.M.F. of amalgamating silver is sensibly the same as that corresponding to the heat of solution of silver in mercury (§ 129). Probably the difference in the cases of silver and cadmium is due to the oxidizability of the latter by dissolved air, thus rendering the outer surface of the crystalline masses somewhat different from the interior.

Cells containing Cadmium opposed to Zinc.

121. The following table exhibits in brief the results of upwards of thirty series of observations, mostly lasting over several hours, during which period the E.M.F. developed by any given cell remained sensibly steady:—

Variation in E.M.F. due to the use of cadmium and zinc sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4\,50\,H_2O$ to $MSO_4\,1250\,H_2O$)	Less than $\pm$·0015 volt.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright cadmium:—				
Fresh electro-metal	−·002	−·007	·005	−·0045
Amalgamated cadmium (liquid)	+·052	+·036	·016	+·044
„ „ (solid, crystalline)	+·024	+·009	·015	+·0165
Electromotive force of combinations:—				
Amalgamated zinc—Bright cadmium	·367	·361	·006	·364
„ „ Electro-cadmium	·362	·358	·004	·360
„ „ Amalgamated cadmium (liquid)	·414	·401	·013	·4075
„ „ Amalgamated cadmium (solid)	·388	·373	·015	·3805

These figures accord closely with the results deducible from Julius Thomsen's thermochemical data, together with the heat of solution of cadmium in mercury above quoted (§ 120); thus:—

	Free metallic cadmium.	Cadmium dissolved in mercury.
Zn, O, SO_3 aq. . . .	=106090	106090
Cd, O, SO_3 aq. . . .	= 89500	88890
Difference . .	16590	17200
Difference per gramme-equivalent . . .	8295	8600
Corresponding with volt	·365	·379

The observed electromotive forces* thus do not differ from

* Regnault has shown (*Ann. de Chim. et de Phys.* [3] xliv. p. 453) that the E.M.F. of a cell containing "concentrated" solutions of zinc and cadmium sulphates and plates of these metals was 55, when that of a similar cell with zinc and copper sulphates and plates was 175 (a particular thermopile being employed as unit). Taking the E.M.F. of the latter cell as 1·115 volt, that of the former must have been ·350 volt—a value differing from those observed by an amount not outside that possibly due to inequality in the molecular strengths of the two metallic solutions. For, by the use of more dilute cadmium-sulphate solutions (the zinc-sulphate solution remaining the same) an appreciable fall in E.M.F. was found to be produced, the lowest value being $42 = ·268$ volt with solution diluted to $\frac{1}{100}$; on the other hand, decreasing the strength of the zinc-sulphate solution produced far less effect. These and the author's somewhat different results on this point will be discussed in a future paper.

those corresponding with the thermal values by amounts materially outside the experimental errors. As with the copper-cadmium cells, however, the observed difference in E.M.F. between electro-cadmium (crystalline) and amalgamated cadmium (liquid) is notably greater than that corresponding with the heat of solution of precipitated crystalline cadmium in mercury, being $\cdot 407 - \cdot 3595 = \cdot 0475$ as compared with ·0135 volt.

Volta's Law of Summation of Electromotive Forces.

122. The foregoing experiments clearly show that, as far as cells containing zinc, cadmium, and copper plates are concerned, Volta's law of summation holds, at any rate when the plates are immersed in solutions of their respective sulphates, the solutions being of equal molecular strength ; that is, the sum of the electromotive forces generated with a given pair of zinc and cadmium plates, and with that same cadmium plate and a given copper plate, is equal to the E.M.F. generated with the given zinc and copper plates; or, otherwise,

$$\left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cd}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cd}\\\mathrm{Cu}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cu}\end{matrix}\right.$$

where the symbol $\left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cd}\end{matrix}\right.$ represents the E.M.F. generated with a given kind of zinc plate opposed to a given kind of cadmium plate, each plate being immersed in a solution of its sulphate of constant molecular strength.

Thus the average results for Daniell cells quoted in Part V. and the above figures give the following comparisons:—

Nature of Plate-surfaces.			Electromotive Force developed.			Normal Daniell.
Zinc.	Cadmium.	Copper.	Zinc-Cadmium.	Cadmium-copper.	Sum.	
Amalgamated.	Electro.	Electro.	·3595	·7525	1·112	Amalgamated zinc and electro-copper 1·111 to 1·116, averaging 1·114.
”	Bright.	”	·364	·7485	1·1125	
”	Amalgamated (fluid).	”	·407	·707	1·114	
”	Amalgamated (solid).	”	·380	·7335	1·1135	
				Mean ...	1·1130	
Amalgamated.	Electro.	Amalgamated.	·3595	·7515	1·111	Amalgamated zinc and amalgamated copper 1·110 to 1·115, averaging 1·113.
”	Bright.	”	·364	·7475	1·1115	
”	Amalgamated (fluid).	”	·407	·706	1·113	
”	Amalgamated (solid).	”	·380	·7325	1·1125	
				Mean ...	1·1120	

A number of direct experiments were also made on this point, using *twin cells* constructed as follows:—Three beakers were arranged containing solutions of copper, cadmium, and zinc sulphates of the same molecular strengths, and plates of electro-copper, bright (or electro-) cadmium, and amalgamated zinc respectively. The copper and cadmium beakers were connected by a siphon tube (with ends covered with bladder) filled with the cadmium sulphate solution; and the cadmium and zinc beakers were similarly connected by a siphon tube containing the zinc sulphate solution. The copper, cadmium, and zinc plates were then connected with cups Nos. 1, 2, and 3 respectively of a switch-board like that represented in fig. 3, Part V. (§ 106); so that by connecting cups 1 and 2 with the electrometer the E.M.F. of the cadmium-copper cell was determined, whilst when cups 2 and 3 were connected the E.M.F. of the cadmium-zinc combination was determined. These readings having been made several times, the zinc and copper plates were transferred to another pair of beakers, containing the same zinc and copper sulphate solutions united by a zinc-sulphate siphon, so as to constitute a normal Daniell cell after Raoult's pattern, and the E.M.F. of this combination determined. Several pairs of zinc and copper plates were thus used—each pair being read first in the zinc-cadmium-copper combination, then in the normal Daniell cell, and then again in the ternary combination. In each case the difference between the sum of the average electromotive forces of the zinc-cadmium and cadmium-copper couples differed from that of the zinc-copper combination by quantities no greater than the errors of observation of the electrometer-scale (about $\pm 0{\cdot}1$ per cent. when a sufficient number of readings were taken); whilst the average of the small differences observed with different pairs was actually 0, *the small + and − differences due to errors of observation completely balancing one another.* This final result (that no discernible difference was to be found between the sum of zinc-cadmium and cadmium-copper couples, and zinc-copper couples containing the same plates) was obtained in each of several sets of experiments made respectively with solutions of molecular strength $MSO_4\,47\,H_2O$, $MSO_4\,100\,H_2O$, and $MSO_4\,1250\,H_2O$.

Rate of Fall in E.M.F. through so-called Polarization occurring in Zinc-Cadmium and Cadmium-Copper Cells for definite amounts of Increase in the Rates of Current-flow.

123. The experiments made with normal Daniell cells described in Part V. (§§ 103–105) were repeated with zinc-cadmium and with cadmium-copper plates (exposing surfaces

of 2·5 and 5·0 square centimetres). The results were similar in character to those obtained with the Daniell cells, no appreciable falling-off in E.M.F. occurring with a current-density of less than some 5 to 10 microampères per square centimetre of plate-surface, but very considerable amounts being observed with stronger currents.

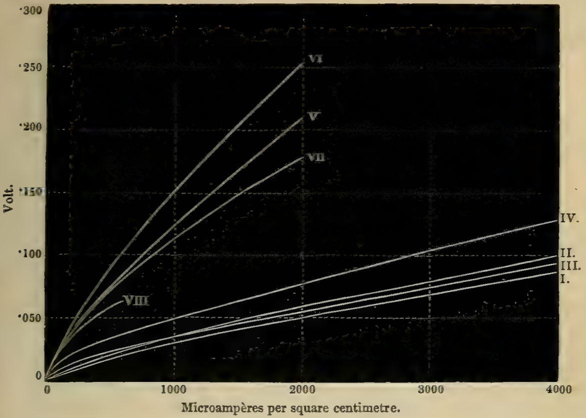
Thus the following table exhibits the values obtained in four experiments—the first three with zinc-cadmium plates, the fourth with cadmium-copper plates, solutions of sulphates of the respective metals employed being used throughout:—

Zinc plates	Amalgamated.	Amalgamated.	Amalgamated.	......
Cadmium plates ...	Electro	Amalgamated. Fluid amalgam.	Amalgamated. Solid amalgam.	Bright.
Copper plates	......	....	....	Electro.
Sp. gr. of zinc sulphate solution...	1·17	1·17	1·17	......
Sp. gr. of cadmium sulphate solution	1·25	1·32	1·32	1·32
Sp. gr. of copper sulphate solution	......	......	......	1·17
Resistance of cell, in ohms	9·5	10·5	11·0	7·4
Maximum E.M.F., in volts	·366	·418	·395	·741
Current-density, in microampères per square centimetre.	Observed amounts of Fall in E.M.F.			
10	0	0	0	·001
20	0	·001	·001	·004
40	·001	·002	·003	·008
100	·002	·004	·007	·014
200	·004	·007	·011	·020
400	·012	·013	·017	·030
1000	·026	·030	·030	·049
2000	·049	·059	·055	·077
4000	·088	·099	·094	·128

The values are represented graphically by the curves marked I., II., III., and IV. respectively in the annexed figure.

A number of experiments were made with cells containing solutions not of equal molecular strengths (like those described in § 110), for the purpose of finding how much of the diminutions observed with the larger current-densities might possibly be due to the strengthening of the solution round the dissolved plate, and the weakening of the liquid round the other plate, which takes place whilst the cell is in action. The general result was that the maximum possible diminution due to these causes could not exceed about ·04 volt. The

details of these experiments and of others allied thereto will be discussed in a future paper.



Effect of varying the Size of one of the Plates, the other remaining constant.

124. The experiments described in Part V. (§ 115) were repeated with various cells containing copper-cadmium or zinc-cadmium plates instead of zinc-copper ones. The following values obtained in four such experiments illustrate the results obtained, indicating that the effect of halving the area of the plate on which metal is deposited is, as with the Daniell cells, greater than the effect of halving the area of the dissolved plate, when the former plate is not mercurialized*. The zinc-cadmium cells, however, differ from ordinary Daniell cells in this respect—that, whilst amalgamating the copper plate of a Daniell cell does not materially alter the relative effect of halving its area, amalgamating the cadmium plate of a zinc-cadmium cell greatly diminishes the relative effect of halving its area, in such sort that, when solid crystalline amalgam is used, the effect of halving the area of the cadmium plates, instead of exceeding, becomes sensibly equal to the effect of halving the area of the zinc plate, whilst when liquid amal-

* It is noteworthy in this connexion, that when nearly pure wrought-iron plates, immersed in ferrous sulphate solution, replace the zinc plates and zinc sulphate solution of normal Daniell cells, the effect of halving the area of the iron plates sometimes *exceeds* that produced by halving the area of the copper plates

gam is used the effect of halving the area of the cadmium plate becomes sensibly less than the effect of halving that of the zinc plate.

Current, in micro-ampères.	Cadmium-Copper.		Zinc-Cadmium.					
	Bright cadmium—Electro-copper.		Amalgamated zinc—Electro-cadmium.		Amalgamated zinc—Fluid amalgamated cadmium.		Amalgamated zinc—Solid amalgamated cadmium.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Cadmium.	Copper.	Zinc.	Cadmium.	Zinc.	Cadmium.	Zinc.	Cadmium.
1000	·005	·008	·004	·005	·005	·003	·005	·006
2000	·010	·013	·007	·008	·009	·005	·011	·015
5000	·016	·022	·012	·013	·018	·011	·021	·019
10000	·020	·031	·020	·025	·027	·015	·026	·025
20000	·039	·051	·030	·040	·035	·022	·032	·032

II. *Cells containing Silver as one of the Metals, the Salts used being Sulphates.*

125. Three sets of cells, after Raoult's pattern, were constructed, containing respectively zinc-silver, cadmium-silver, and copper-silver couples, the respective plates being immersed in solutions of silver sulphate saturated at ordinary temperatures, and of copper and zinc sulphate of strengths molecularly equal thereto (the silver solution contained 7·25 grammes of Ag_2SO_4 per litre, and had a sp. gr. near to 1·0067); the composition was uniformly $MSO_4\,2360\,H_2O$.

On making series of determinations of the E.M.F.'s of these cells, the following results were arrived at as the effects of varying the nature of the silver surfaces, deduced from the average values of a large number of observations (upwards of 50 sets of comparisons).

Effect of substituting Electro-Silver for Bright Silver.

	Zinc opposed.	Cadmium opposed.	Copper opposed.
Maximum ...	+·017	+·015	+·012
Minimum ...	+·003	+·005	+·004
Range.........	·014	·010	·008
Average	+·008	+·009	+·0075

Effect of substituting Amalgamated Silver (fluid) for Bright Silver.

Maximum ...	+·102	+·110	+·105
Minimum ...	+·092	+·095	+·092
Range.........	·010	·015	·013
Average......	+·099	+·101	+·1025

Considering the perceptibly wider ranges of variation in these experiments than those usually observed in the zinc-cadmium-copper cells previously described, it is evident that the effect of varying the nature of the silver plate is sensibly independent of the nature of the other metal.

Sometimes, but not invariably, the amalgamated silver plates became solid and crystalline (greenish yellow) on the surface: this result was apparently brought about much more rapidly when the silver was immersed in concentrated zinc sulphate solution than under any other of the conditions obtaining in the various experiments. When this change took place the E.M.F. set up by opposing such a plate to zinc in cells containing sulphates of zinc and silver was always intermediate between that set up in the same fluids by plates of bright silver and of fluid amalgamated silver. The average of several comparisons was as follows:—

Effect of substituting Amalgamated Silver (solid) for Bright Silver.

Maximum............	+·025
Minimum............	+·014
Range	·011
Average	+·021

In this respect silver is analogous to cadmium when the latter is opposed to zinc (§ 121); but the average amounts of increase in E.M.F. due to fluid and solid amalgam are in each instance considerably greater with silver than with cadmium (·099 and ·021 for silver as compared with ·043 and ·016 for cadmium).

On substituting electro- for bright cadmium in the cadmium-silver cells, identically the same average effect was observed as in the cadmium-copper cells (§ 119), viz. an increase in the E.M.F. of from ·002 to ·006, averaging ·004 volt. Similarly, on substituting amalgamated for electro-copper in the copper-silver cells, practically the same numerical difference in the E.M.F. was brought about as was formerly observed in the zinc-copper cells (§ 107) and the cadmium-copper cells (§ 119), but in the opposite direction, the E.M.F. being *raised* in the

copper-silver cells and lowered in the other two kinds: the alteration in the E.M.F. varied between +·005 and −·003, averaging +·0005.

126. On allowing newly set-up cells to stand for several hours, different results were brought about in each of the three cases according as the silver was opposed to zinc, cadmium, or copper. In the first case the E.M.F. invariably fell; the maximum value was observed immediately after the cell was set up, and continued sensibly steady for a variable period of time, a distinct diminution becoming perceptible sometimes after half an hour, sometimes only after two or three hours. With cadmium the value after several hours was somewhat greater than that set up at first and during the subsequent hour or so; and with copper the value attained after several hours was still greater than that exhibited during the first hour or two. The following numbers represent the average alterations thus observed, being the differences between the average readings during the first hour and during a period of from 3 to 5 hours after setting up:—

Bright silver.	Electro-silver.	Amalgamated silver.	Mean.
	Amalgamated Zinc opposed.		
−·010	−·014	−·012	−·012
	Bright Cadmium opposed.		
+·003	+·001	+·003	+·002
	Electro-Cadmium opposed.		
+·003	+·004	+·003	+·003
	Electro-Copper opposed.		
+·009	+·007	+·007	+·008
	Amalgamated Copper opposed.		
+·011	+·009	+·005	+·008

These alterations were traced to the variations in the nature of the surfaces of the plates opposed to the silver; for on taking out, for instance, an amalgamated zinc plate after 5 hours, and replacing by a freshly amalgamated plate, the E.M.F. was restored to sensibly the same value as at first; and similarly with the other metals. On the other hand, on taking out from two cells, for instance, a zinc and a copper plate after 5 hours, and replacing them respectively in two beakers containing zinc and copper sulphate solutions of the same molecular strengths, and connected by a siphon tube, the E.M.F. of the cell thus formed was found to fall short of the

average value of a normal Daniell cell with fresh plates by an amount sensibly equal to the sum of the numerical alterations that had occurred in the zinc-silver and copper-silver cells jointly. It is specially noticeable that, whilst in zinc-copper cells the alteration in the surface of the copper (probably through oxidation) on standing diminishes the E.M.F., in copper-silver cells the alteration is in the opposite direction: with zinc and cadmium the direction of this alteration when opposed to silver is the same as when opposed to copper.

It would seem from all these results that the effect of a given alteration of the surface of one of the plates of a voltaic pair upon the E.M.F. of the pair is independent of the nature of the other plate as regards its numerical value, although the nature of this second plate regulates the direction of the variation in the E.M.F. produced (increase or decrease), and also exerts an influence upon the rate at which the alteration of the plate-surface takes place. Thus it was repeatedly observed that, whereas an amalgamated zinc plate (or an electro-copper one), when forming part of a normal Daniell cell, did not become sensibly oxidized, so as to diminish the E.M.F. of the cell, until after several hours at least had elapsed, a precisely similar plate immersed in the same liquid, but forming part of a zinc-silver cell (or of a copper-silver cell), did become perceptibly oxidized in much less time. In other words, although no measurable current was generated in either case, yet the different amounts of strain (so to speak) set up in the chain of liquid particles between the two plates, according as one was silver or not, did affect the rate of change in the surface of the more oxidizable metal (presumably by varying the rate at which it combined with the oxygen dissolved in the fluid).

127. The following Table exhibits the average results obtained in about 150 sets of observations and comparisons, only those values made during the first hour (or sometimes less) after setting up being taken into account, and all subsequent values being rejected where any diminution through oxidation &c. began to be perceptible *. The values cited as the average effects of substituting for bright silver electro- and amalgamated (liquid) silver are the means of the three sets above quoted (§ 125) obtained respectively with zinc, cadmium, and copper:—

* Notwithstanding all care, it is probable that the average results with the zinc-silver cells are too low by a few thousandths of a volt, and those with the other cells slightly too high (vide § 128).

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright silver:—				
Electro-silver	+·017	+·003	·014	+·008
Amalgamated silver (liquid)	+·100	+·092	·008	+·101
„ „ (solid)	+·025	+·014	·011	+·021
Effect of substituting for electro-copper:—				
Amalgamated copper	+·005	−·003	·008	+·001
Effect of substituting for bright cadmium:—				
Electro-cadmium	+·006	+·002	·004	+·004
Electromotive force of combinations:—				
Amalgamated zinc—Bright silver	1·540	1·518	·022	1·528
„ „ Electro-silver	1·550	1·529	·021	1·536
„ „ Amalg. silver (liquid)	1·640	1·615	·025	1·627
„ „ „ „ (solid).	1·555	1·544	·011	1·549
Bright cadmium—Bright silver	1·173	1·163	·010	1·1675
„ „ Electro-silver	1·185	1·169	·016	1·1765
„ „ Amalg. silver (liquid).	1·278	1·261	·017	1·2685
Electro-cadmium—Bright silver	1·176	1·164	·012	1·1715
„ „ Electro-silver	1·186	1·176	·010	1·1805
„ „ Amalg. silver (liquid)	1·277	1·267	·010	1·2725
Electro-copper—Bright silver	·422	·411	·011	·416
„ „ Electro-silver	·429	·411	·018	·4235
„ „ Amalg. silver (liquid)	·535	·513	·022	·5185
Amalgamated copper—Bright silver	·420	·411	·009	·4165
„ „ Electro-silver	·430	·414	·016	.424
„ „ Amalg. silver (liquid)	·535	·513	·022	·519

Volta's Law of Summation.

128. The values in this table, together with those quoted above for the zinc-cadmium and cadmium-copper cells (§§ 119 and 121) and those given in Part V. for zinc-copper cells (§ 107), clearly prove that Volta's law holds in the case of the sets of combinations

$$(A) \quad \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Cd}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cd}\\ \mathrm{Ag}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Ag}\end{matrix}\right.$$

$$(B) \quad \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Cu}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cu}\\ \mathrm{Ag}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Ag}\end{matrix}\right.$$

$$(C) \quad \left\{\begin{matrix}\mathrm{Cd}\\ \mathrm{Cu}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cu}\\ \mathrm{Ag}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Cd}\\ \mathrm{Ag}\end{matrix}\right.$$

at any rate within the range of possible error due to the somewhat larger ranges of fluctuation in the E.M.F. of silver-containing cells than were observed with zinc-cadmium-copper cells, and to the fact that, although alterations of the oxidizable plates in the cells containing silver was avoided as far as possible by only carrying on the observations for one hour and sometimes less, still it was not practicable wholly to avoid this source of inaccuracy. Thus in the three cases respectively the following figures are obtained :—

Case A.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Zinc.	Cadmium.	Silver.	Zinc-cadmium.	Cadmium-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Bright.	Bright.	·364	1·1675	1·5315	1·528	+·0035
,,	Electro.	,,	·3595	1·1715	1·531		+·003
,,	Bright.	Electro.	·364	1·1765	1·5405	1·536	+·0045
,,	Electro.	,,	·3595	1·1805	1·540		+·004
,,	Bright.	Amalgamated (liquid).	·364	1·2685	1·6325	1·627	+·0055
,,	Electro.	,,	·3595	1·2725	1·632		+·005
						Average difference =	+·0042

Case B.

Zinc.	Copper.	Silver.	Zinc-copper.	Copper-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Electro.	Bright.	1·114	·416	1·530	1·528	+·002
,,	Amalgamated.	,,	1·113	·4165	1·5295		+·0015
,,	Electro.	Electro.	1·114	·4235	1·5375	1·536	+·0015
,,	Amalgamated.	,,	1·113	·424	1·537		+·001
,,	Electro.	Amalgamated (liquid).	1·114	·5185	1·6325	1·627	+·0055
,,	Amalgamated.	,,	1·113	·519	1·632		+·005
						Average difference =	+·0027

Case C.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Cadmium.	Copper.	Silver.	Cadmium-copper.	Copper-silver.	Sum.	Cadmium-silver.	Difference.
Bright.	Electro.	Bright.	·7485	·416	1·1645	1·1675	—·003
,,	Amalgamated.	,,	·7475	·4165	1·164		—·0035
,,	Electro.	Electro.	·7485	·4235	1·172	1·1765	—·0045
,,	Amalgamated.	,,	·7475	·424	1·1715		—·005
,,	Electro.	Amalgamated (liquid).	·7485	·5185	1·267	1·2685	—·0015
,,	Amalgamated.	,,	·7475	·519	1·2665		—·002
Electro.	Electro.	Bright.	·7525	·416	1·1685	1·1715	—·003
,,	Amalgamated.	,,	·7515	·4165	1·168		—·0035
,,	Electro.	Electro.	·7525	·4235	1·176	1·1805	—·0045
,,	Amalgamated.	,,	·7515	·424	1·1755		—·0050
,,	Electro.	Amalgamated (liquid).	·7525	·5185	1·271	1·2725	—·0015
,,	Amalgamated.	,,	·7515	·519	1·2705		—·0020
					Average difference	=	—·0032

Average difference in case A	...... =	+·0042
" " " B	...... =	+·0027
" " " C	...... =	−·0032
Mean	.... =	+·0012

On carrying out twin-cell experiments like those described in § 122, it was found that when a silver plate was placed in the central beaker, and either copper and zinc, copper and cadmium, or zinc and cadmium plates were used in the other beakers, together with solutions of the respective metallic sulphates of the same molecular strengths, the difference between the electromotive forces determined in the twin cell was always sensibly equal to the E.M.F. developed by the pair of plates other than silver employed when taken out and opposed to each other in an ordinary cell containing the same metallic solutions; and this was found to be the case, not only with freshly-prepared plates, but also with plates that had been immersed for hours and had become oxidized on the surface. For instance, in a pair of experiments with amalgamated zinc, bright silver, and electro-copper plates:—

	Cells newly set up.	After 24 hours.
Zinc-copper in single cell . .	1·115	1·098
Copper-silver in twin cell . .	·417	·425
Sum	1·532	1·523
Zinc-silver in twin cell . . .	1·534	1·522
Difference . .	−·002	+·001

Similar results were obtained in several other experiments of the same kind, both with zinc-copper plates opposed to silver and with the other two pairs (zinc-cadmium and cadmium-copper). The average difference in each case was considerably less than $\pm$·001 volt.

Relations between the Electromotive Forces of Zinc-Silver, Copper-Silver, and Cadmium-Silver Cells and those corresponding to the net Chemical Actions taking place therein.

129. In all the cells hitherto examined in this series of researches there has been shown to be a sensible equality between the electromotive forces generated with clean plate-surfaces of pure metals and those corresponding to the net chemical and physical actions taking place therein. *A priori,*

there does not seem to be any evident reason why the same state of things should not exist in the case of cells containing silver plates. Thomsen's thermo-chemical valuations, however, indicate that the electromotive forces corresponding to the three equations

$$\begin{aligned} Zn &+ Ag_2SO_4 = 2Ag + ZnSO_4, \\ Cd &+ Ag_2SO_4 = 2Ag + CdSO_4, \\ Cu &+ Ag_4SO_4 = 2Ag + CuSO_4, \end{aligned}$$

are considerably higher than those actually observed and tabulated above, thus:—

Zinc-silver.		Cadmium-silver.		Copper-silver.	
Zn, O, SO_3 aq. =	106090	Cd, O, SO_3 aq. =	89500	Cu, O, SO_3 aq. =	55960
Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390
Diff., per gramme-molecule	85700		69110		35570
Diff., per gramme-equivalent.........	42850		34555		17785
Corresponding to volt	1·890		1·524		·784

In each of the three cases the calculated E.M.F. is from ·34 to ·37 volt above the observed values when silver plates not amalgamated are used, indicating that even when only infinitesimal currents are generated a large amount of energy is nonadjuvant. It will be shown in a future paper that this behaviour is more or less marked in other kinds of cells containing silver plates and silver compounds, although the minimum amount of nonadjuvancy observed in any given case is variable with the nature of the saline compounds in the cell*. In order to see whether the rise in E.M.F. produced by amalgamating the silver plate is due simply to the heat of formation of silver sulphate being less when the silver is dissolved in mercury than when it is free, determinations of the heat of solution of silver (precipitated from the nitrate by copper, crystalline) were made. It was found a little difficult to get every trace of silver used dissolved in mercury when 20 grams of silver and 600 of mercury were employed, even though the surface of the former was washed with dilute nitric acid. With smaller amounts of mercury only incomplete solution was

* Raoult has already obtained numbers (*Ann. Chim. et Phys.* [4] ii. 317 and iv. 392) indicating that the "galvanic heat" (§ 17) of a cell containing copper, silver, and the nitrates of their metals is sensibly below the value due to the net chemical action taking place. These and other similar observations will be discussed in a future paper.

effected, more or less pasty amalgam being formed, and a smaller heat-development being then noticeable. The most trustworthy determinations made indicated that during the solution of 108 grams of silver, 2070 gramme-degrees are evolved. Hence the heat of formation of silver sulphate, when the silver is dissolved in mercury, is, per gramme-molecule, $20390 - 2 \times 2070 = 16250$; so that the heat of displacement of silver from silver sulphate, when the metal is ultimately obtained in mercurial solution, is greater than the values above calculated for zinc, cadmium, and copper respectively as precipitating metals by 4140 gramme-degrees per gramme-molecule, or 2070 per gramme-equivalent, corresponding with ·0915 volt. The average difference in E.M.F. between cells otherwise alike, in which crystalline electro-silver and amalgamated silver (fluid) are respectively used, is almost identical with this, being $\cdot 101 - \cdot 008 = \cdot 093$ volt. As already noticed (§ 120), silver and cadmium differ in their behaviour in this connexion when amalgamated, the inequality between the effect on the E.M.F. actually produced by amalgamation of cadmium and that corresponding with the heat of solution in mercury being probably due to the oxidation of cadmium by dissolved air.

Rate of Fall in Electromotive Force through so-called "Polarization" for definite Amount of Increase in Rate of Current-flow.

130. In order to see whether the electromotive forces of the cells above described are rendered less (when no current passes) than they otherwise would be, through the interfering action of dissolved air or some other similar obscure cause, a number of experiments were made like those described in Part V. (§§ 103–105), with the general result of showing that the deficiency observed in the E.M.F. actually generated when no current passes (as compared with that calculated from the thermo-chemical data) is at any rate not due to any such cause, inasmuch as the E.M.F. generated when a current does pass is always more or less *below* that set up when no current passes, just as with normal Daniell cells and with the zinc-cadmium and cadmium-copper cells above described. Thus, for example, the following numbers were obtained in three experiments, in each of which bright zinc plates were employed, and one in which electro-copper plates were used, the silver plates being in each instance uppermost and surrounded by saturated silver sulphate solution.

Fluid surrounding the zinc plates.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·10.	......
Fluid surrounding the copper plates	......	......	......	Copper sulphat sol., sp. gr. 1·17
Nature of silver plates.	Electro-silver (crystalline.	Solid crystallized amalgam.	Solid crystallized amalgam.	Electro-silver (crystalline).
Resistance, in ohms, of column of fluid between plates	66·9	76·0	76·6	71·0
Maximum E.M.F.	1·500	1·511	1·546	·401
Current-density, in microampères.	Observed amounts of fall in Electromotive Force.			
20	......	......	·012	·005
50	·016	·010	·021	·011
100	·025	·018	·026	·019
200	·041	·036	·037	·030
400	·062	·067	·059	·047
600	·080	·097	·077	·063
1000	·116	·144	·112	
2000	·209	·258	·177	

These numbers are represented graphically by the curves marked respectively V., VI., VII., and VIII. in the figure.

On trying experiments, like those described in § 110, to see how far the falling-off in E.M.F. when a current is generated could be due to the accumulation of zinc (cadmium or copper) sulphate round the plate opposed to the silver, it was found that the maximum possible effect due to this cause could not exceed about ·04 volt with zinc and cadmium, and ·02 with copper. These experiments, and others of a similar nature, will be discussed in a future paper.

On comparing the eight curves represented in the figure with those previously described as obtained with various forms of Daniell cell (Part V. § 105), it is noticeable, first, that the curves obtained with the zinc-cadmium cells underlie all the others (I., II., and III.); secondly, that the curve with the cadmium-copper cell (IV.) is practically identical with one of the Daniell-cell curves—indicating consequently that, whilst the substitution of copper for cadmium in a zinc-cadmium cell raises the position of the curve (*i. e.* increases the rate of fall in E.M.F. according as the current-density increases), the substitution of cadmium for zinc in a Daniell cell does not materially alter the position of the curve ; thirdly, the curves with the zinc-silver and copper-silver cells overlie all the others, whilst the copper-silver curve (VIII.) is not widely different from the zinc-silver curves (V., VI., and VII.)—indicating that, whilst the substitution of silver for copper in a

Daniell cell largely raises the position of the curve, the effect of substituting copper for zinc in a zinc-silver cell is very much less marked. In other words, *the nature of the dissolved metal affects the rate of decrease in E.M.F. with increasing current-density much less than does the nature of the deposited metal; whilst the less the heat of formation of the salt of the latter that is decomposed by the passage of the current, the more rapid appears to be the rate of fall in the E.M.F. of the cell as the current-density increases.* As regards the first part of this general conclusion, it is precisely what also results from the majority of the previously described experiments on the effect of halving the area of the dissolved plate, as compared with that produced by halving the area of the plate on which metal is deposited. The following experiments with cells containing silver plates also give the same general results.

Effect of Varying the Size of one of the Plates in Cells containing Silver as one of the Metals, the other Plate remaining unaltered.

131. By operating in the way described in § 115, the following results were obtained in four sets of observations with zinc-silver and copper-silver cells, showing that, in all cases, halving the area of the silver plate produces a sensibly greater decrease in the E.M.F. set up with a constant rate of current-flow than is effected by halving the area of the plate opposed to the silver.

Current-density, in micro-ampères.	Zinc-Silver.						Copper-Silver.	
	Amalgamated zinc—electro-silver.		Amalgamated zinc—crystalline amalgamated silver.		Amalgamated zinc—crystalline amalgamated silver.		Electro-copper—electro-silver.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Zinc.	Silver.	Zinc.	Silver.	Zinc.	Silver.	Copper.	Silver.
1000	·009	·011	·007	·026	·008	·019	·004	·011
2000	·012	·024	·009	·045	·012	·036	·010	·023
3000	·014	·038	·011	·059	·015	·047	·017	·028
5000	·016	·064	·020	·103	·021	·070		
8000	·028	·093	·038	·186	·029	·122		
10000	......	......	......	......	·034	·151		

Summary of Results.

132. The foregoing results may be thus summarized:—

Cells containing zinc and cadmium or cadmium and copper

plates, immersed in solutions of the sulphates of these metals respectively, are closely analogous to ordinary Daniell cells (containing zinc sulphate solution). Slight variations in the E.M.F. generated are introduced by varying the condition of the plate-surfaces; but in all cases the maximum E.M.F. actually generated with clean pure plate-surfaces and with solutions of equal molecular strengths is close to that calculable from the net chemical action taking place in the cell when generating a current. When the cadmium plates are not amalgamated, or are covered with crystalline solid amalgam, the electromotive forces are close to ·75 and ·36 volt for cadmium-copper and zinc-cadmium cells respectively, the values corresponding to the net chemical actions as deduced from Thomsen's thermo-chemical results being substantially the same. When the cadmium plates are covered with fluid amalgam, the electromotive forces are lower in the first case and higher in the second by upwards of ·04 volt—a quantity distinctly exceeding in magnitude the E.M.F. corresponding with the heat of solution of cadmium in mercury, although of the same sign.

(2) The electromotive forces of zinc-silver, cadmium-silver, and copper-silver cells containing the respective sulphates of these metals differ from those of zinc-cadmium, zinc-copper (Daniell), and cadmium-copper cells in this respect, that the maximum electromotive forces generated (the fluids being of equal molecular strength) are not sensibly the same as those calculated from Julius Thomsen's thermal data, but in every case fall short by an amount not far from ·35 volt. When the silver plates are not mercurialized, or are coated with crystalline amalgam, the electromotive forces (which vary slightly with the precise nature of the plate-surfaces) are, in the three cases, near to 1·53, 1·17, and 0·42 volt respectively, the metallic solutions being of equal molecular strength. When the silver plates are covered with fluid amalgam, the electromotive forces are in each case about ·09 volt higher than the values obtained with electro-deposited crystalline metal, this increase almost exactly coinciding with the increment corresponding with the heat of solution of silver in mercury.

(3) As long as the cadmium and zinc [or copper] solutions employed are of the same molecular strength within the limits indicated respectively by $MSO_4\,50H_2O$ and $MSO_4\,1250H_2O$, the E.M.F. developed with a given pair of cadmium and zinc [or copper] plates is sensibly independent of the actual strength of the solutions, these cells behaving precisely like Daniell cells in this respect. With Daniell cells the solutions are practically of the same molecular strength when they

are of the same specific gravity; but with the other cells containing cadmium this is not so, cadmium-sulphate solution being uniformly more dense than either zinc or copper solution of the same molecular strength.

(4) The effect on the E.M.F. of a cell of a given alteration in the nature of the surface of either a zinc, copper, cadmium, or silver plate is sensibly the same numerically whichever other one of these four metals be opposed to it; but the direction of the alteration is opposite according as the plate is the anode or the kathode of the combination.

(5) Volta's "Law of Summation" universally holds within the limits of experimental error in all the cases examined; that is, the electromotive forces of zinc-cadmium, cadmium-copper, and copper-silver combinations are such that, for any given kinds of plate-surfaces, the sums of the two first, of the two last, and of the three together are respectively equal to the electromotive forces of zinc-copper, cadmium-silver, and zinc-silver combinations.

(6) Zinc, copper, and cadmium plates alter superficially (probably in consequence of oxidation by dissolved air) more rapidly when opposed to silver than when opposed to any other one of these four metals, on being immersed in solutions of their respective sulphates forming one half of a cell on Daniell's principle—no current being generated by the cell, the measurements being made by means of a quadrant electrometer.

(7) With all the cells examined the behaviour when generating a current is analogous to that of a normal Daniell cell: when the current-density exceeds a few microampères per square centimetre of plate-surface, a more or less marked diminution in the E.M.F. ensues, the falling-off being the greater the greater the current-density. With moderately strong currents the diminution far exceeds the maximum possible amount due to accumulation of dissolved salt round the plate dissolved, and exhaustion of solution round the other plate. *Cæteris paribus*, the rate of fall in E.M.F. as the current-density increases is the more rapid the lower the heat of formation of the metallic salt decomposed in the cell so as to deposit the metal, and is comparatively but little affected by the nature of the dissolved metal.

(8) The effect of halving the area of the plate on which metal is deposited is usually to cause a greater diminution in the E.M.F. than is produced by halving the area of the dissolved plate; amalgamated cadmium plates in zinc-cadmium cells, however, form an exception to this rule.

XXIII. *An Integrating Anemometer.* By WALTER BAILY*.

[Plate V.]

THE object of the instrument described in this paper is to resolve the velocity of the wind in two directions at right angles to one another, and to obtain the time-integral of each part separately.

The instrument contains a horizontal plane, in which are two slits N S and E W, forming a cross to be placed with its arms towards the cardinal points. In these slits are sliders F, G, connected by a bar of constant length. O is the centre of the cross, H the centre of the bar. The locus of H is a circle with centre O. A weathercock or some equivalent mechanism is to keep H in such a position that the radius O H is in the direction of the wind. The sliders carry beneath them wheels, B, C, whose planes are perpendicular to their respective slits, and whose centres are beneath the pivots joining the slits to the bar. [See figs. 1, 2, 3, Plate V. Fig. 1 gives a perspective view of the instrument, omitting some points; fig. 2 gives a view of the top of the instrument; and fig. 3 gives a section of a slit and slider, and shows the wheel carried by the slider.] The wheels B, C rest on a disk, A (fig. 1), which revolves about a vertical axis immediately below O. The disk A is to be rotated by Robinson's cups, or some equivalent mechanism, so as to have a velocity proportional to that of the wind. The pieces which carry the wheels B, C should be allowed some play in a vertical direction; and the contact of B and C with A can then be maintained either by their own weight or by the use of a spring. The number of rotations of B in a given time is proportional to the time-integral of the resolved part of the wind in one direction (say, north); and the number of rotations of C is proportional to the time-integral of the resolved part of the wind in a direction at right angles to the first (say, west).

Let Ω be the angular velocity of the disk A; ω, ω' the angular velocities of the wheels B, C; m, m' the number of their rotations in a given time t; b their radius, a the length of the bar; θ the angle between the direction of the wind and (say) the north; then $b\omega = a\sin\theta\,.\,\Omega$, and $b\omega' = a\cos\theta\,.\,\Omega$; and the integrals required are

$$\int_0^t \Omega\sin\theta\,dt = \int_0^t \frac{b}{a}\omega\,dt = \frac{b}{a}\pi m$$

* Communicated by the Physical Society, having been read at the Meeting on June 10, 1882.

and

$$\int_0^t \Omega \cos\theta \, dt = \int_0^t \frac{b}{a} \omega' dt = \frac{b}{a} \pi m'.$$

Therefore m, m' are proportional to the required integrals.

Each slider might carry a train of wheels to record the number of rotations; or an electrical arrangement might be made in which each wheel should complete a circuit at each rotation and the number of contacts should be recorded. In the latter case, as no distinction is preserved as to the direction in which the wheels revolve, it becomes necessary to have four circuits, one for each cardinal point, with a recorder in each, and to have one connected with each arm of the cross.

A working model of the instrument above described was exhibited at the Meeting, and was fitted with an electrical arrangement such as I have mentioned.

I have since discovered that the slits, sliders, and bar above described may be replaced by a train of cogged wheels. (Fig. 4 represents the upper, and fig. 5 the under surface of the train.) A bar turns in a horizontal plane about O, and is kept in the direction of the wind. This bar carries three wheels, H, K, L, having the same axis. The length of the bar from one pivot to the other is supposed to be an inch and a half. The wheels H, K are rigidly connected; and L lies between them and turns independently. H and K are 1 inch, and L is 3 inches in diameter. L rolls on the inner edge of P, and H rolls on the inner edge of Q, the diameters of P and Q being 6 and 4 inches respectively. Two wheels, M and N, whose diameters are 2 inches, are carried by the wheel L, and have their centres at the extremities of a diameter of L. M and N are in the same plane as K, and are therefore touched by it. As the bar rotates, M and N move without rotation, and their centres move in straight lines passing through O at right angles to one another, and are at a fixed distance apart, and have the line joining them bisected by the bar, which is the direction of the wind. Hence M and N may be used to carry the wheels B, C (fig. 1) instead of their being carried by the sliders F and G.

XXIV. *On the Effect upon the Ocean-tides of a Liquid Substratum beneath the Earth's Crust.* *By the* Rev. O. Fisher, *M.A., F.G.S.**

(1) In a work which I have lately published, entitled 'Physics of the Earth's Crust' †, I have maintained the theory that the crust is thin, and floats in equilibrium upon a slightly

* Communicated by the Author.

† Macmillans, 1881.

denser substratum of molten rock, the elevations on the surface of the crust being due to compression, and being supported through flotation by corresponding protuberances (which I call "roots of the mountains") projecting downwards into the denser liquid—a mode of support long ago suggested by Sir G. B. Airy *.

The most formidable difficulty in the way of this theory has been said to be the necessary occurrence of tides in such a substratum; and it has been thought that, the crust being carried up and down in sympathy with the substratum, the ocean-tides would be almost entirely masked, and that there would be no appreciable rise and fall of the water relatively to land †.

The explanation of the difficulty which I had offered in the book itself was, that the tides in the substratum would involve a horizontal transference of fluid backwards and forwards, and might be expected to be of small amplitude, owing to the viscosity of the substance and to its confinement beneath the crust ‡; and I felt so convinced, from geological considerations, that the substratum must be at least plastic, if not liquid, that I did not think it needful to go further into the question. But in consequence of the weight of authority by which this objection has since been enforced, I have been induced to examine it more closely, and to endeavour to discover what indications, if any, the ocean-tides might be expected to give of the existence, or otherwise, of such a substratum.

(2) The "canal theory" of the tides appears to be the most suitable to solve, in a general way, the question at issue; for what we have to do is to investigate the motion of layers of liquids (the substratum and the ocean) under the influence of tide-producing forces.

An article upon the tides by Mr. D. D. Heath appeared in this Magazine § in 1867. He intimates that it was founded upon Airy's treatise in the *Encyclopædia Metropolitana*. I shall take the liberty of adopting the introductory paragraphs of his analysis, merely changing a symbol. The liquid is treated as "confined to a narrow channel running round the equator, supposing the moon vertical over it and moving uniformly in her orbit, so that her apparent motion will be also uniform and somewhat less than that of the earth's rotation."

"§ 3. And first as to the geometrical characteristics of a fluid wave uniformly propagated westward at any rate (α).

* Phil. Trans. Roy. Soc. vol. cxlv. p. 101.

† 'Nature,' vol. xxv. p. 423, 1882; also New-York 'Nation,' June 15, 1882.

‡ 'Physics of the Earth's Crust,' p. 23.

§ Fourth Series, vol. xxxiii. p. 165, March 1867.

"Taking any point on the equator as origin, let x measure the longitude [linear] westward of any other point, and let κ be the mean depth of the water [or liquid] and y the small elevation of the surface above the level at x as a definite moment of time.

"For the wave to be propagated with a persistent form and at the rate α, the height at x must, in a time dt, change from y to the value which y now has at a distance αdt behind it, or $x-\alpha dt$ from the origin; that is,

$$\frac{dy}{dt}dt + \&c. = -\frac{dy}{dx}\alpha dt + \&c.$$

or

$$\frac{dy}{dt} = -\alpha\frac{dy}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

And if this relation exist everywhere between the differential coefficients, the condition will be fulfilled for finite intervals.

"Not only the heights, but every other measure or mark of disturbance must be propagated onwards at the same rate, if the wave is to have a permanent character; so that if v be the average forward velocity of the particles in a vertical section at x, we must have

$$\frac{dv}{dt} = -\alpha\frac{dv}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)"$$

It is assumed in Mr. Heath's paper, and, I believe, usually, that κ, the depth of the canal, is "but a very few miles." This assumption is avoided in what follows, not being compatible with the problem we have to solve; for although it will appear that the assumption that κ is small might have been made, yet this could not have been readily foreseen.

(3) The assumptions which will be made are:—That the horizontal velocity of all the particles of water in a vertical column are the same; that the vertical and horizontal velocities are small, and that the elevation or depression of the surface above or below the mean level is also small; so that products of these quantities and of their derivatives may be neglected.

From this it follows that, expressing partial differential coefficients by brackets, since

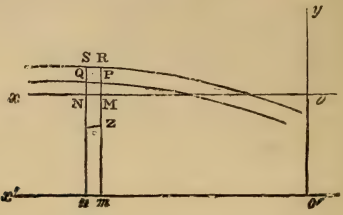
$$\frac{dv}{dt} = \left(\frac{dv}{dt}\right) + \left(\frac{dv}{dx}\right)v,$$

$$\therefore \quad \frac{dv}{dt} = \left(\frac{dv}{dt}\right), \text{ approximately;}$$

and

$$\therefore \quad \left(\frac{dv}{dt}\right) = -\alpha\left(\frac{dv}{dx}\right).$$

Henceforth the brackets will be omitted.



Let $O\,x$ be the mean level of the surface,
$O\,O'$ be the depth of the canal $=\kappa$,
$O\,M = x$,
$M\,P = y$,
$m\,Z = z$.

Let w be the vertical velocity of the liquid at Z at the time t, and in the time dt let the surface rise from P Q to R S.

Then the volume of liquid which enters Q Z in the time dt is

$$v(\kappa + y - z)dt + w\,dx\,dt\,;$$

and that which goes out is

$$v'(\kappa + y' - z)dt,$$

v' and y' being the values of v and y at $x + dx$, or

$$v + \frac{dv}{dx}dx, \text{ \&c.}$$

And their difference is SP, or

$$\frac{dy}{dt}dx\,dt.$$

Therefore, neglecting the products vy and $v'y'$, we get

$$w - \frac{dv}{dx}(\kappa - z) = \frac{dy}{dt}.$$

It is obvious that when $z=0$, then $w=0$, and $\frac{dy}{dt}$ is not a function of z; whence the two relations,

$$-\kappa\frac{dv}{dx} = \frac{dy}{dt}, \quad \ldots\ldots\ldots \quad (1)$$

and
$$w = -\frac{dv}{dx}z. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

But we know (A) that $\frac{dy}{dt} = -\alpha\frac{dy}{dx}$.

Wherefore from (1),

$$\frac{dv}{dx} = \frac{\alpha}{\kappa}\frac{dy}{dx};$$

$$\therefore \quad \frac{dw}{dx} = -\frac{\alpha}{\kappa}\frac{d^2y}{dx^2}z;$$

and also
$$v = \frac{\alpha}{\kappa}y. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

By the same reasoning as that by which we have concluded that $\frac{dv}{dt} = -\alpha\frac{dv}{dx}$, we likewise conclude (B) that

$$\frac{dw}{dt} = -\alpha\frac{dw}{dx},$$

$$= \frac{\alpha^2}{\kappa}\frac{dy^2}{dx^2}z. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

(4) The equations of fluid motion in two dimensions will be, ρ being the density and p the pressure at Z:—

$$\begin{cases} \frac{1}{\rho}\frac{dp}{dx} = X - \frac{dv}{dt} - v\frac{dv}{dx} - w\frac{dv}{dz}, & . \quad . \quad . \quad . \quad (1) \\ \frac{1}{\rho}\frac{dp}{dz} = Z - \frac{dw}{dt} - w\frac{dw}{dx} - w\frac{dw}{dz}. & . \quad . \quad . \quad . \quad (2) \end{cases}$$

In our problem ρ is constant. Considering a tide formed over a rigid bottom, the horizontal forces are the difference between the moon's horizontal attractions at the point x, z, and in a parallel direction at the earth's centre, and the friction.

The moon's differential horizontal attraction is $-\frac{3aM}{2D^3}\sin 2\omega$, M being her mass, a the earth's radius, D the moon's mean distance, and ω the longitude west of the moon's meridian. It will be negative, because it acts in an easterly direction. Call it $-\mu \sin 2\omega$.

The horizontal friction, if taken as proportional to the velocity v of the liquid (which is true of low velocities), f being the coefficient of friction, is fv, or (by (3) of § 3), $f\frac{\alpha}{\kappa}y$. When

y is positive, or the liquid flowing westward, this force acts eastward, and *vice versâ*; so that its sign is always opposite to that of y. It will therefore be expressed by $-f\frac{\alpha}{\kappa}y$. Hence

$$X=-\mu\sin 2\omega-f\frac{\alpha}{\kappa}y;$$

also

$$Z=-g.$$

Therefore, neglecting the small products, and substituting for $\frac{dv}{dt}$ and $\frac{dw}{dt}$ the values lately found, the equations are reduced to

$$\left\{\begin{array}{ll}\frac{1}{\rho}\frac{dp}{dx}=-\mu\sin 2\omega-f\frac{\alpha}{\kappa}y+\frac{\alpha^2}{\kappa}\frac{dy}{dx}, & \quad\ldots\ldots (1)\\ \frac{1}{\rho}\frac{dp}{dz}=-g-\frac{\alpha^2}{\kappa}\frac{d^2y}{dx^2}z. & \quad\ldots\ldots (2)\end{array}\right.$$

Integrating (2), and taking the integral from 0 to z, we obtain

$$\frac{1}{\rho}p=g(\kappa+y-z)+\frac{\alpha^2}{2\kappa}\frac{d^2y}{dx^2}\left((\kappa+y)^2-z^2\right);$$

∴ differentiating with respect to x, and neglecting the small products,

$$\frac{1}{\rho}\frac{dp}{dx}=g\frac{dy}{dx}+\frac{\alpha^2\kappa}{2}\frac{d^3y}{dx^3}.$$

If there should be a floating crust of any kind capable of adapting itself freely to the wave, the pressure arising from this cause must be added to p; but it disappears when differentiated.

Hence, putting $a\omega$ for x, and equating the values of $\frac{1}{\rho}\frac{dp}{dx}$, the differential equation to the wave-surface is

$$f\frac{\alpha}{\kappa}y+\left(g-\frac{\alpha^2}{\kappa}\right)\frac{1}{a}\frac{dy}{d\omega}+\frac{\alpha^2\kappa}{2a^3}\frac{d^3y}{d\omega^3}=-\mu\sin 2\omega.$$

Assume

$$y=A\cos 2\omega-B\sin 2\omega,$$

$$\therefore\ y=\sqrt{A^2+B^2}\cos(2\omega+2\delta),$$

where

$$\tan 2\delta=\frac{B}{A}.$$

And we find

$$\tan 2\delta=\frac{f\frac{a\alpha}{\kappa}}{2\left(g-\frac{\alpha^2}{\kappa}\right)-4\frac{\alpha^2\kappa}{a^2}};\quad\ldots\ (3)$$

whence

$$y = \frac{a\mu}{2\left(g - \frac{\alpha^2}{\kappa}\right) - 4\frac{\alpha^2}{a^2}\kappa} \cos 2\delta \cos(2\omega + 2\delta).$$

(5) Suppose c to be the maximum value of y when friction is not taken account of. Then

$$c = \frac{a\mu}{2\left(g - \frac{\alpha^2}{\kappa}\right) - 4\frac{\alpha^2}{a^2}\kappa}.$$

And the maximum value of y when friction acts will be

$$c' = c \cos 2\delta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

Equations (3) and (4) show that, as friction is increased, 2δ tends towards 90° or δ towards 45°; while at the same time c', the maximum tide above mean level, diminishes to zero. Hence as friction (or viscosity) increases, the vertex of the tidal spheroid moves eastward, the ellipticity of the tidal spheroid simultaneously decreasing, until, when friction is infinite, its vertex reaches 45° east of the moon, and the tide disappears altogether. The same general result appears from Mr. Darwin's table (p. 16)* to hold good in the case of bodily tides, if there should be such in the earth.

(6) We notice that our result is independent of the density of the liquid, and that the weight of a floating crust, if considered flexible, would not affect it—the reason being that such a crust would aid in depressing the hollows just as much as it would hinder the elevation of the ridges. It would have an effect analogous to an additional load to the bob of a pendulum.

The coefficient of κ in the denominator of the expression for c shows that the term may be neglected, although κ itself be not small. For α is the space over which the wave travels in one second, while a is the radius of the earth.

Neglecting this term, c and likewise c' are positive or negative according as

$$\kappa > \text{ or } < \frac{\alpha^2}{g}.$$

Consequently, when $\kappa < \frac{\alpha^2}{g}$ there will be low tide under the

* "On the Bodily Tides of Viscous and Semi-elastic Spheroids, and on the Ocean-Tides upon a yielding Nucleus," Phil. Trans. Roy. Soc. part i. 1879.

moon, and when $\kappa > \frac{\alpha^2}{g}$ there will be high tide under the moon. In the case of the semidiurnal tide $\frac{\alpha^2}{g}$ is about 12 miles.

When $\kappa = \frac{\alpha^2}{g}$ the result fails; for c becomes infinite, which is contrary to the assumptions on which the solution has been obtained.

(7) Let us now look to the effect of a tide in the crust of the earth upon the ocean-tide, to see whether the tide formed in a liquid substratum would so far diminish the ocean-tide that the observed amount of the ocean-tide would disprove the existence of a liquid substratum. The manner in which such a diminution of the ocean-tide would be produced in an extreme case appears thus:—Suppose that the earth were liquid, and that there were an extensible film within it at a depth from the surface equal to the ocean depth. Then, on the equilibrium theory, the entire sphere would be deformed as a whole, and the measurable tide would be merely the excess of the deformation at the surface beyond that at the depth at which the film lay; which excess would be inappreciable.

In considering this question, it is necessary to take account of the attraction upon the ocean of the part of the tidal earth-spheroid exterior to the sphere to which it is tangential. The problem has been worked out by Mr. G. H. Darwin in part ii. of his paper "On the Bodily Tides of a Viscous Spheroid"*. He considers the moon as moving uniformly in the equator and raising tide-waves in a narrow equatorial canal. The greatest range of the bodily tide is taken as 2E; and it is supposed to be retarded after the passage of the moon by an angle $\frac{\epsilon}{2}$, which corresponds to δ in this paper. The expression at which he arrives for the motion of the wave-surface of the ocean relatively to the bottom of the canal (observing that he measures the ordinate downwards instead of upwards as I have done), when the symbols are replaced by those here used, becomes

$$\kappa + \frac{\kappa}{\alpha^2 - gk}\left\{\left(\frac{a\mu}{2}\cos 2\delta - \frac{2}{5}g\mathrm{E}\right)\cos(2\omega + 2\delta) + \frac{a\mu}{2}\sin 2\delta \sin(2\omega + 2\delta)\right\}.$$

The apparent tide relatively to land can therefore be written,

* Phil. Trans. Roy. Soc. part i. 1879, p. 22.

putting, as before, $\frac{a\mu\kappa}{2(\alpha^2-g\kappa)}=c$,

$$h=c\left\{\cos 2\omega-\frac{4}{5}\frac{g\mathrm{E}}{a\mu}\cos(2\omega+2\delta)\right\}.$$

To find its maximum height put $\frac{dh}{d\omega}=0$;

$$\therefore\ 0=c\left\{-\sin 2\omega+\frac{4}{5}\frac{g\mathrm{E}}{a\mu}\cos(2\omega+2\delta)\right\}.$$

Squaring, and adding, and calling the high tide H, we have

$$\mathrm{H}^2=c^2\left\{1-\frac{4}{5}\frac{g\mathrm{E}}{a\mu}\left(2\cos 2\delta-\frac{4}{5}\frac{g\mathrm{E}}{a\mu}\right)\right\},$$

from which the diminution of the ocean high tide by the earth-tide, or $c-\mathrm{H}$, can be found.

(8) The canal theory of the tides is doubtless less applicable to the fortnightly than to the semidiurnal tide. Nevertheless a certain fortnightly tide would be raised in an equatorial canal; and since we are seeking the *pro ratâ* diminution only, and not the absolute height of the tide, we may assume that this result will be applicable to the fortnightly tide if we assign corresponding values to the symbols. It is evident that the earth-tide must be of the same period as the ocean-tide which is affected by it; so that H, c, and E will belong to tides of the same period.

The foregoing equation is suitable to find the diminution of the ocean-tide by the earth-tide, whether we consider the tidal deformation of the ocean-bottom to arise from a bodily tide in a non-rigid, but solid, earth, or from a tide in a liquid layer beneath the crust, and of a depth which is small compared with the radius. The latter is the theory of the constitution of the earth which I have maintained in my 'Physics of the Earth's Crust,' and against which the tidal argument has been held to present a formidable objection. It is therefore with this supposition that I am concerned. The canal theory of the tides, dealing with a layer of liquid, seems to be the suitable one upon which to estimate the deformation of the crust through the tide in the liquid substratum—that is, to give the value which we ought to assign to E. Let, then, η be the tide which would be formed in such a substratum were it of uniform depth, perfectly liquid, and frictionless. Then, from what has been already proved,

$$\mathrm{E}=\eta\cos 2\delta.$$

Now

$$\eta = \frac{a\mu}{g} \frac{\kappa}{2\left(\kappa - \frac{\alpha^2}{g}\right) - 4\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}}.$$

Substituting for E, and observing that $\frac{a\mu}{g}$ divides out, we have

$$H^2 = c^2\left\{1 - \frac{2}{5}\frac{\kappa\cos^2\delta}{\kappa - \frac{\alpha^2}{g} - 2\frac{\kappa^2}{a^2}\frac{\alpha^2}{g^2}}\left(2 - \frac{4}{5}\frac{\kappa}{\kappa - \frac{\alpha^2}{g} - 2\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}}\right)\right\}.$$

This expression is therefore applicable to find the diminution of an ocean-tide of any period, if we give a corresponding value to α, because the moon's force is not involved in it*.

(9) The theory of "mountain-roots," described at the commencement of this paper, requires that the liquid substratum should be at least about 60 miles deep. Now if α be the velocity of the semidiurnal tide, $\frac{\alpha^2}{g}$ is about 12 miles. Hence κ will be at least five times $\frac{\alpha^2}{g}$. And if we omit the consideration of friction, then $\cos 2\delta = 1$, and $\frac{\kappa^2}{a^2}\frac{\alpha^2}{g}$ is very small, so that

$$H = c\{1 - \tfrac{2}{5}\tfrac{5}{4}\} = \tfrac{1}{2}c.$$

The semidiurnal tide would therefore, under these circumstances, be diminished to one half.

If the depth of the substratum were, say, 120 miles, the tide, when friction is neglected, would be diminished by $\frac{4}{9}$, or it would have $\frac{5}{9}$ of its undiminished value.

(10) But, no doubt, friction would have a very considerable effect upon a tide of short period in such a substance as molten rock, confined both above and below between a rigid nucleus and the floating crust. The factor $\cos^2 2\delta$ would therefore greatly lessen the term expressing the diminution of the tide. The mountain-roots likewise, following the contour of the land masses of the globe, would so confuse the tidal phenomena, both in the substratum and in the ocean, that little

* "If Ω be the moon's orbital velocity, and I the inclination of the plane of the orbit to the earth's equator, then the part of the tide-generating potential on which the fortnightly tide depends is

$$\frac{9}{8}\frac{m}{c^3} wr^2 \sin^2 I(\tfrac{1}{3} - \cos^2\theta)\cos 2\Omega t,"$$

where m is the moon's mass, c her distance, w the mass per unit of volume of the earth, r the radius vector, and θ the colatitude (Darwin, "On the Bodily Tides," &c., *loc. cit.* p. 15).

reliance can be placed upon the above estimates. It is, however, necessary to remark that all the accidents to which I have referred would tend to diminish the earth-tide, and to lessen the consequent diminution of the ocean-tide, and so to weaken any argument against the theory of a liquid substratum derived from the circumstance that a diminution of the semidiurnal tide has never been detected.

(11) It appears, however, to be chiefly upon the fortnightly tide that reliance is placed for answering the question whether the earth is rigid or not, because friction and the interference of continents will have a much smaller effect upon that slow tide. Now the fortnightly tide in an equatorial canal would have a velocity of about 54 feet per second. If we substitute this value for α, neglecting the effect of friction, and putting $\kappa = 60$ miles, we obtain

$$H = c \times 0{\cdot}6.$$

If we use a larger value for κ, the result will be but little affected.

(12) The effect of friction would not altogether disappear even in the case of the fortnightly tide; and the supposed mountain-roots, which appear to be a necessary accompaniment of a liquid substratum, would still interfere with the formation of the tide, and diminish the value of E, and lessen the diminution of the ocean-tide.

Suppose, then, that if there were no friction in the substratum, the ocean-tide would, as shown above, be diminished to 0·6 of its undiminished value. Let us inquire what the coefficient of friction in the substratum would need to be to bring the ocean-tide up to 0·7 of its undiminished value. Now

$$H = c\{1 - \tfrac{2}{5}\eta \cos^2 2\delta(2 - \tfrac{4}{5}\eta)\} \text{ nearly.}$$

Hence the diminution varies as $\cos^2 2\delta$. By our supposition

$$\frac{1-0{\cdot}7}{1-0{\cdot}6} = \cos^2 2\delta,$$

or

$$2\delta = 30^\circ.$$

Now (§ 4)

$$\tan 2\delta = \frac{fa\alpha'}{2(g\kappa - \alpha'^2)};$$

and α', the equatorial velocity of the fortnightly tide, $= 54{\cdot}28$ feet per second; $\kappa = 60 \times 5280$ feet; and $a = 209 \times 10^5$ feet; $g = 32{\cdot}2$ feet. From this equation we find

$$f = 0{\cdot}0178 \tan 2\delta.$$

And if
$$2\delta = 30^\circ,$$
$$f = 0{\cdot}010316,$$
$$= \tfrac{1}{100} \text{ nearly.}$$

What is meant by such a coefficient of friction may be thus realized. Mr. Heath remarks that it is only necessary to consider the friction on the bottom of the canal. The effect is assumed to be to check equally all the liquid in each vertical column, as if it were all directly subject to the action of the bottom (and of the crust at the top also in the case of the substratum). This, as he says, "can hardly be true, though probably the resulting calculation exhibits something closely resembling the case of nature; for it is the *average* forward velocity on which the form of the wave depends." But it seems to me that the reciprocatory nature of the motion of the liquid will nearly confine the effect of friction to the two surfaces, because scarcely will the retardation have been propagated far before it will be reversed in direction. The case will be somewhat analogous to the propagation of seasonal variations of temperature into the earth's surface.

We have then, integrating the equation

$$\frac{dv}{dt} = -fv,$$

$$t = \frac{1}{f}\,\text{h.l.}\,\frac{C}{v},$$

where C is the initial velocity. Whence, if $f = 0{\cdot}01$, such an amount of friction would reduce the velocity to one half in $100 \times \text{h.l.}\ 2$, or $69{\cdot}3$ units of time. For instance, if a stream of the material were flowing over level ground with a velocity of one mile an hour, it would be reduced to half a mile an hour in $69{\cdot}3$ hours. As the layer of liquid may, from what has just been said, be likened to a solid layer whose upper and under surfaces are lubricated by a sufficient thickness of the liquid, this would seem to be a low degree of viscosity for such a substance as molten rock.

In the case of the semidiurnal tide $\alpha = 1520$ feet per second; and if we put $f = 0{\cdot}01$, 2δ will be about 87°, and $\cos^2 2\delta = 0{\cdot}0024$. This factor will render the diminution of the ocean-tide practically inappreciable.

(13) The final result therefore is that, upon the canal theory of the tides, if there be a liquid substratum of 60 miles or more in depth beneath the crust, and resting on a rigid nucleus, upon the supposition (of course impossible) that the

substance is perfectly without friction, the fortnightly ocean-tide in an equatorial canal would be reduced to about 0·6 of its calculated amount. It would require a coefficient of friction of 0·01 to bring this tide up to 0·7 of its calculated value. But the same amount of friction would have so great an effect in reducing the semidiurnal tide in the substratum, that the ocean-tide of that period would not be perceptibly affected. This is entirely in accordance with the *à priori* reasoning suggested in my book, as referred to at the beginning of this article. Without friction, however (a condition impossible in nature), the semidiurnal tide would be reduced to one half its calculated value.

Should then observations on the fortnightly tide lead to the conclusion that it is reduced to somewhere about 0·7 of its calculated value, it would appear that such a result would on the canal theory agree perfectly well with the theory of a liquid substratum upon a rigid nucleus. And, further, under these circumstances no appreciable diminution of the semidiurnal ocean-tide could be expected.

The entire range of the fortnightly tide at Teneriffe, upon the supposition of a rigid earth, would be, according to Sir William Thomson, 4·5 inches*.

August 12, 1882.

XXV. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* By J. J. Thomson.

To the Editors of the Philosophical Magazine and Journal.

Trinity College, Cambridge,
August 21st, 1882.

Gentlemen,

I INFER from Prof. Clausius's letter in the last Number of the Philosophical Magazine that he has misunderstood my position with regard to the question of the dimensions of a magnetic pole in the electrostatic system of units. I did not attempt to show that Maxwell's value was in accordance with Ampère's theory as Prof. Clausius interprets it, but endeavoured to show that Maxwell's value was the necessary consequence of the principles laid down in his treatise; and to point out what modification of Ampère's theory these principles lead to. From what Prof. Clausius says about Ampère's theory being independent of electrodynamic considerations, it would seem that he understands the theory to state that every small magnet *is* an electric current, and not that the magnetic effects of every small magnet may be represented by those of

* Natural Philosophy, § 845, ed. 1867.

an electric current, since this statement is only intelligible on electrodynamic considerations. The theory in this form seems rather hypothetical for the foundation of a system of units. Prof. Clausius does not think it objectionable that his formula for the magnetic force between two poles is not in the electrostatic system of units of the dimensions of a force. But since the attraction between two poles is as much a force as the attraction of the sun on the earth, if the expression mm' / r^2 is not of the dimensions of force on the electrostatic system, it is clear that this formula no longer represents the force between two poles, and that another factor must be introduced to make the expression of the right dimensions. Now it is one of the great advantages of Maxwell's system that all his formulæ are true as they stand, and do not require the arbitrary introduction of a factor on passing from one system of units to another; these factors introduce themselves naturally through symbols representing some physical property of the body or medium. As Maxwell does not dwell on this point in his book, I may be pardoned if I quote a few illustrations of it.

Using the notation of Maxwell's treatise, the force between two electrified particles $= \frac{ee'}{\kappa r^2}$, where κ is the specific inductive capacity of the substance. Now in the electrostatic system the dimensions of e are $(M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1})$, and κ is of no dimensions in mass, space, and time. In the electromagnetic system the dimensions of e are $M^{\frac{1}{2}}L^{\frac{1}{2}}$, and κ is of dimensions $L^{-2}T^2$. Thus in both these systems the expression $ee' / \kappa r^2$ is of the dimensions of force; and the factor changing from the one system to the other makes it appearance in the κ.

The same thing is true for the force between two magnetic poles. The expression for the force is $mm' / \mu r^2$, where μ is the magnetic permeability of the substance. In the electrostatic system m is of dimension $[M^{\frac{1}{2}}L^{\frac{1}{2}}]$, and μ of dimensions $[L^{-2}T^2]$; in the electromagnetic system m is of dimension $[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}]$, and μ of no dimensions; thus in both systems the expression $mm' / \mu r^2$ is of the dimensions of force. Other illustrations might be given; but the reader can easily verify the statement that in Maxwell's system every equation is true as it stands; and consequently, whenever we have a purely dynamic effect, the expression for it will be of the same dimensions in both systems of units.

I am, Gentlemen,

Your obedient servant,

J. J. Thomson.

XXVI. *A new Form of Magnetic Torsion-balance and Magnetometer.* By FREDERICK JOHN SMITH*.

IN the torsion-balance, such as used by Coulomb to measure magnetic forces, two poles of the suspended magnet are acted on. The end the author has had in view is so to place one pole of the suspended magnet that it shall not be acted on by horizontal pull. A magnet of rectangular shape, N B S, fig. 1, having a brass counter-weight A B, is suspended by the filaments C D so that the south pole is in the axis of rotation of the whole mass; thus the magnet may be regarded as having only one pole that can be acted on by horizontal force. In addition to this, a little mirror is so placed on an axis, O M, and attached to S by a short lever P, that the ratio of the deflection of N to the rise of the whole mass is at once shown on the scale, R, by the usual reflecting method. In the magnetometer for determining the pole-strength of magnets the same kind of rectangular magnet is used, attached to a single horizontal wire, A B, fig. 2; the magnet is furnished with a mirror-scale and lamp.

Fig. 1.

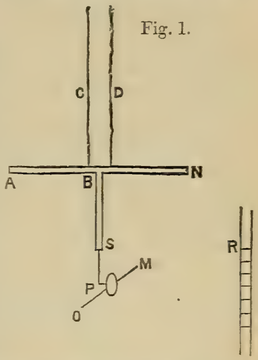
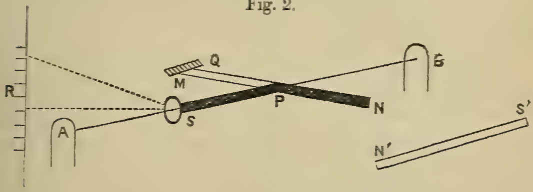


Fig. 2.



N P S, rectangular magnet; S, mirror; R, scale and lamp.

The instrument is used thus:—The weight at Q and the magnet N′ S′ being removed, the magnet N P S is set in a horizontal position by means of the torsion-wire; then a known weight, Q, is placed at M, M P = P N, and the deflection on the scale is recorded; then the weight is removed, and the

* Communicated by the Author.

same deflection produced by bringing up the N pole of the magnet N′S′. The magnets are made of equal pole-strength and tested in the usual manner; the distance between them is taken in centimetres, the weight Q being in parts of the gramme. In the formula which shows the relationship between magnetic forces, $f=\frac{m \times m'}{d^2}$ (where $f=$ force, $m\,m'$ pole-strengths, d distance between them), if $m=m'$ as in this instrument, then $f=\frac{m^2}{d^2}$; and if f be replaced by Qg (*i. e.* the numerical value of the weight $\times$ gravitation), the formula becomes $Qg=\frac{m^2}{d^2}$, from which m is known in absolute measure. With the pole-strength thus obtained the strength of other magnets and solenoids can be readily compared. In order to secure great strength, the frame which carries the torsion-wire is a gun-metal casting, having a rib at the back.

Taunton, July 27, 1882.

XXVII. *Notices respecting New Books.*

A Treatise on the Distillation of Coal-Tar and Ammoniacal Liquor. By George Lunge, *Ph.D., F.C.S., Professor of Technical Chemistry in the Federal Polytechnic School, Zurich.* London: Van Voorst, 1882.

Dr. Lunge is so well known, not only from his published investigations, but also from his 'Treatise on the Manufacture of Sulphuric Acid and Alkali,' that we were desirous of ascertaining how he would handle the subjects of coal-tar and ammoniacal liquor. A careful examination of his book enables us to pronounce most favourably upon it; and we regard it as a very valuable contribution to the literature of coal-tar and its derivatives.

Dr. Lunge commences his work by a preliminary chapter of 25 pages entitled "The Origin of Coal-tar," in which he discusses the differences between the tars derived from peat, browncoal, bituminous coal, and real coal, and also the effect of the temperature at which the coal is distilled upon the resulting products. We quite coincide with the author that the system well-nigh universally adopted, of squeezing the maximum quantity of gas out of the coal and letting the tar come out as it may, is a bad system. We may, perhaps, be hardly prepared to accept the dictum recently uttered at the annual meeting of gas-managers by a well known American, that, before long, gas will become the "residual" and coal-tar the principal object in the carbonization of coal; nevertheless it is, we think, indisputable that coal-tar is a necessity of the age, and will become rapidly of greater and greater value.

In Chapter II. a table is given of the constituents of coal-tar.

This table is the most complete that has yet been published. It must be remembered, however, that it includes the products of Boghead and other bituminous shales as well as those of true coals. After the table, is given a brief but sufficient account of the chemical and physical characters of the principal substances mentioned in it. The sketch of the history and properties of benzene is very complete, and includes a clear description of the views of its constitution propounded by Kekulé, Claus, and Ladenburg. The author is quite right in saying that it would be well-nigh impossible to find one's way through the interminable field of the aromatic compounds, if Kekulé's theory of the constitution of benzene had not brought light and order into it.

Chapter III. is devoted to "The Applications of Coal-tar without Distillation." It is well written and very interesting, and describes the various attempts that have been made to utilize it for fuel, varnishes, &c.; it is, however, becoming too valuable for these and similar methods of getting rid of what was once regarded as a nuisance.

The chapter on the "First Distillation of Coal-tar" is so minute that there is even a section on the "Carriage of Coal-tar." Even the best form of casks is discussed; and we are informed that "Long barrels are said to be pulled more easily than those more bellied." Does the author mean rolled instead of "pulled"? The sentence seems rather obscure. The distillation of tar by steam and fire is fully described; and the best forms of stills, condensers, and rectifying apparatus are illustrated by engravings.

In treating of Pitch, the manufacture of artificial asphalt, and of the so-called asphalt-pipes used for conveying water, acid, air-blasts, for covering underground telegraph-wires, &c. &c., is described, and instructions are given for the preparation of artificial fuel. The distillation of pitch for the production of anthracene oil is fully treated; and engravings are given of Fenner and Versmann's apparatus.

Chapter VI. is devoted to the working-up of the anthracene oil, which is effected by cooling to cause the solid hydrocarbons to deposit; the whole is then pressed, and the liquids are returned to the heavy oils, or employed as lubricants, or are redistilled. The solid portion constitutes rough anthracene, and may be sold as such, or be submitted to further purification. The preparation of anthracene has, however, been very fully treated of by Auerbach, whose excellent treatise has been translated into English, and is in the hands of every one interested in the subject. The chapter on Creosote oils is a valuable one; but we think the author is scarcely sufficiently alive to the comparatively small part played by phenol in the preservation of wood. Certain it is that long after the phenol has so far disappeared that it is almost impossible to detect it even by the most delicate tests, the wood continues to remain sound for many years. We are also of opinion that those modern specifications for creosote oils which insist that no naphthalene shall be deposited at 40° F. have been issued under a false impression of

what constitutes a good creosote oil, and would exclude oils of the highest preservative character. We have lately examined timber "pickled" 30 years ago with creosote oils from the London tar, and which oils when cooled to 40° F. yielded a very large amount of naphthalene; and yet the wood remains perfectly sound to this day.

The directions for the preparation and estimation of pure phenol (carbolic acid) are very minute, and include the results of the most recent workers on the subject. The author also describes the methods of preparing carbolic soap and disinfecting powders.

The chapter on Ammoniacal Liquor is well written, and contains a full account of all the substances contained in it; directions are also given for estimating its value, and for working it up. This latter part of the work is fully illustrated with engravings.

In conclusion, we congratulate the author upon having produced a work which is absolutely indispensable to all manufacturers of coal-tar products.

Worked Examination Questions in Plane Geometrical Drawing. *By* F. E. Hulme. Longmans, Green, and Co.: London.

This work consists of three hundred questions taken from old examination papers, two thirds of which have figures corresponding to them, said to be *solutions* of the problems. There is no attempt at classification; on the contrary, it has been purposely avoided by the author, and for a reason with which we do not hold. There are no demonstrations, and in most cases only scant directions; and even these are given where least, and omitted where most, needed. Many of the constructions are empirical, and incapable of being demonstrated; hence, from a mathematical point of view, they are not solutions at all. Scale Questions, those bugbears of Candidates for Military Examinations, ought to have been collected, and complete solutions of typical cases given. What good results from answering precisely similar questions over and over again? Notwithstanding its many defects, there is much in the book to recommend it. The questions are such as are certain to be encountered in Woolwich and Sandhurst papers; and the constructions are well drawn and conveniently placed for easy reference.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE DURATION OF THE PERCEPTION OF LIGHT IN DIRECT AND INDIRECT VISION. BY AUG. CHARPENTIER.

After various experimenters, I have sought to determine the time that elapses between the appearance of a light before the eye and the making of a signal by the subject of the experiment as soon as he perceives the light. There was interest in ascertaining if the duration of the perception was different for the centre and for the excentric portions of the retina, if exercise could modify that duration, and if the modification would or would not be limited to the part exercised.

For these experiments the eye, place at the centre of a Landolt's perimeter, looked into a large box lined with black, in the bottom of which a perforation had been made, about 1 square centim. in

section, usually closed by a plate lined with black, which plate, heavy and metallic, was retained in its position by the attraction of an electromagnet, but without coming into immediate contact with the latter, so that as soon as a person placed behind the box interrupted the current animating the electromagnet the stopping-plate instantly fell and disclosed the window placed before the eye which was under experiment. A current supplied by a laboratory Gramme machine, after passing through the electromagnet, put in action a small Deprez signal, the pen of which left its trace on a registering cylinder with a Foucault regulator. The signal immediately announced the interruption of the current, and consequently the precise moment of appearance of the light. Then the subject under experiment, directly after perceiving the light, restored the current in the signal through a derived path, by pressing on a spring the index finger of his right hand; precisely at this moment a new sign is traced upon the registering cylinder.

The interval which had elapsed between the interruption and reestablishment of the current, measured by comparison with the vibrations of a Marey electric chronograph, indicated directly the time which had been required for the subject to perceive and signal the light. For shortness, I shall call that time simply the duration of the luminous perception.

Here are the principal results which I have obtained in this investigation:—

(1) For one and the same person, under the same conditions, the duration of the perception varies from single to double without any apparent regularity. But if in one and the same experiment the mean of a sufficiently large number of successive determinations be taken (ten for example), a duration constant during the whole time of the experiment is found. I have found for myself, in direct vision, a mean duration of 0·13 second with daylight.

(2) The duration of the direct perception varies according to the individuals. I have seen it vary, according to the persons, from 0·09 to 0·15 second.

(3) The duration of the perception is sensibly the same for the right and for the left eye when they are sound.

(4) The duration of the luminous perception is notably increased by another cerebral occupation imposed on the subject during the experiment. Thus, when he speaks, when he listens attentively to a reading or a discourse, while at the same time applying himself to the experiment, he must have, for the reaction, 0·04 or 0·06 second more than before.

(5) The duration of the luminous perception is always more considerable in indirect than in direct vision; it is more considerable in proportion as the point of the retina struck by the light is more distant from the centre. This cannot be due to a difference of sensitivity, since, as I together with M. Landolt have shown, the retina is everywhere nearly equally sensitive to light.

(6) The difference between the duration of indirect and that of direct vision showed itself especially considerable at the beginning of our experiments. There was then between the duration of per-

ception for the centre and for a point situated 80° on the outer side in the visual field a difference of nearly 7 hundredths of a second. That difference was notably lessened by repetition of the same experiments during a month and a half; at the end of that time it was not more, for my left eye, than 2 hundredths of a second.

(7) If exercise attenuates the difference of duration of direct and indirect perception, it never annihilates it; so that the first constantly takes place more rapidly than the second. The influence of exercise asserts itself rapidly from the first sittings; afterwards it takes effect rather slowly, and then affects direct as well as indirect vision.

(8) Having established at the commencement that the duration of perception is the same for the left as for the right eye, I made, almost every day during a month and a half, fifty determinations on two well-defined points of my left eye only, excluding all other points of my two retinas. I thus exercised exclusively, a very great number of times, the centre of the *left* eye and the point of the *left* retina corresponding to 80° in the external part of the visual field (the internal part of the retina). At the end of that time I could estimate the influence of exercise by comparing the duration of the luminous perception on the same points in the *right* retina, and even on other points in both retinas. That duration was, for the centre of the left eye 0·129 second, for the centre of the right eye (not exercised) 0·143; at 80° outside for the left eye the duration of perception was 0·160 second; at 80° outside, for the eye not exercised, 0·210 second. Therefore exercise had notably shortened the duration of the reaction of the points experimented on.

(9) I wished to see if the abbreviating influence had extended over the *left* eye to points which had not been exercised. Now the duration of the reaction *was found to have been shortened in the same proportion for all the points of the inner half of the left retina* (the outer side of the visual field), *but not for the points of the outer half.* Consequently the exercise of an excentric point affects the different points of the same retinal hemisphere, but not those of the other hemisphere.

(10) The shortening influence *had extended to the outer hemisphere of the retina of the right eye*, while *the inner hemisphere reacted much more slowly than the same part, exercised, of the left eye.*

These facts can hardly be explained, except by admitting Wollaston's theory respecting the incomplete crossing of the fibres of the optic nerve in the chiasma, and supposing that the exercise of one part of the retina does not act merely on that part itself, but rather on the whole of the nervous centre, which receives both the fibres from the half of the retina containing the exercised point and the fibres from the half *on the same side* of the opposite retina.

Most of these experiments were simultaneously made by my assistant M. Bernardy, who aided me throughout, but, unfortunately, being able only to utilize the right eye for these researches, did not control points 8 and 10.—*Comptes Rendus de l'Académie des Sciences*, July 10, 1882, t. xcv. pp. 96–99.

AN AIR-THERMOMETER WHOSE INDICATIONS ARE INDEPENDENT OF THE BAROMETRIC PRESSURE. BY ALBERT A. MICHELSON.

The appearance of an abstract of a paper by Pettersson* on a new air-thermometer has led me to publish, sooner than I had contemplated, a notice of an instrument far simpler and more manageable than that which is there described, and which likewise retains the important advantage of giving indications which are independent of the external pressure.

The instrument consists of a glass bulb and stem, the former about 40 millim. and the latter about 2 millim. in interior diameter. The bulb contains dry air at a pressure of about 100 millim. of mercury; and this air is separated from the upper portion of the tube by a column of mercury about 100 millim. in length. The mercury remains above the air, notwithstanding the large diameter of the bore, owing to the resistance to deformation of the meniscus. The space above the mercury is a vacuum.

Thus the pressure of the air in the bulb is constant, and is equal to that of the column of mercury above it. If the bore of the stem is not of uniform section, the length of the column will change; but this length is easily read off, and gives at once the true pressure.

The pressure need not be limited to 100 millim.; but if it be much greater the instrument becomes inconveniently long.

The only precaution to be observed, beyond what is used in an ordinary mercurial thermometer, is that the stem must be kept vertical.—Silliman's *American Journal*, August 1882.

Case School of Applied Science,
Cleveland, O., July 5, 1882.

ON A PROPERTY OF THE ISENTROPIC CURVE FOR A PERFECT GAS AS DRAWN UPON THE THERMODYNAMIC SURFACE OF PRESSURE, VOLUME, AND TEMPERATURE. BY FRANCIS E. NIPHER†.

The equation of this thermodynamic surface is

$$pv=\mathrm{RT}; \quad \ldots\ldots\ldots\ldots \quad (1)$$

where p, v, T represent the pressure, volume, and absolute temperature, and where R is directly proportional to the volume of a unit mass (or inversely proportional to the density) of the gas at a standard temperature and pressure.

By differentiation, (1) becomes

$$dp=\frac{\mathrm{R}}{v}d\mathrm{T}-\frac{\mathrm{RT}}{v^2}dv. \quad \ldots\ldots\ldots\ldots \quad (2)$$

For convenience, putting

$$\frac{\mathrm{R}}{v}=\mathrm{A}, \quad \frac{\mathrm{RT}}{v^2}=\mathrm{B},$$

(2) becomes

$$dp=\mathrm{A}d\mathrm{T}-\mathrm{B}dv. \quad \ldots\ldots\ldots\ldots \quad (3)$$

1. To find the direction of maximum slope with respect to the v, T plane at any point on the surface. For this purpose pass a

* *Annalen der Physik und Chemie* (Beiblätter), No. 5, 1882.

† From Trans. of St. Louis Academy of Sciences, read April 3, 1882.

plane through any point in the surface, and at right angles to the v, T plane. Its trace upon the v, T plane is

$$T=\beta+av, \quad \ldots\ldots\ldots\ldots \quad (4)$$

p being indeterminate, where a is the tangent of the angle which the trace makes with the v axis, or

$$a=\frac{dT}{dv}. \quad \ldots\ldots\ldots\ldots \quad (5)$$

From (3) and (5) we have

$$dp=(Aa-B)dv. \quad \ldots\ldots\ldots\ldots \quad (6)$$

Calling S the slope of any element of the intersection of the plane and the surface, dz being the projection of the element on the v, T plane, we have

$$S=\frac{dp}{dz}=\frac{dp}{\sqrt{dv^2+dT^2}}, \quad \ldots\ldots\ldots\ldots \quad (7)$$

which by (5) becomes

$$S=\frac{dp}{dv}\cdot\frac{1}{\sqrt{1+a^2}};$$

and by (6) we have, further,

$$S=\frac{Aa-B}{\sqrt{1+a^2}}. \quad \ldots\ldots\ldots\ldots \quad (8)$$

In determining the direction of maximum slope at any point, it is evident that A and B will be constant, which gives as the required condition,

$$\frac{dS}{da}=\frac{A+Ba}{(1+a)^{\frac{3}{2}}}=0,$$

or

$$a=-\frac{A}{B}.$$

Substituting the values of A and B, we have

$$a=-\frac{v}{T}=-\frac{R}{p}=\tan i. \quad \ldots\ldots\ldots\ldots \quad (9)$$

For very low pressures, the direction of maximum slope $\frac{dp}{dz}$ becomes more and more nearly at right angles to the plane of p, v; while for high pressures this direction becomes more and more nearly parallel to the plane of p, v. The direction of maximum slope is constant along a line of constant pressure.

2. To find the direction of the isentropic line at any point on the surface, as related to the direction of maximum slope determined in (9).

Poisson's equation,

$$Tv^{k-1}=\text{const.}, \quad \ldots\ldots\ldots\ldots \quad (10)$$

is a projection of the isentropic line upon the plane of v, T, where k is the ratio of the specific heats $=1{\cdot}41$.

Calling a' the tangent of the angle which any element of this

projection makes with the v axis, we have

$$a'=\frac{dT}{dv}=\tan i'.$$

This value of a' is obtained by differentiating (10), and is found to be

$$\frac{dT}{dv}=-\frac{T}{v}(k-1)=-\frac{P}{R}(k-1). \quad\dots\dots\dots\dots \quad (11)$$

Here also the condition of constant pressure gives a constant value for a'. Hence, at any point along any line of constant pressure, the production of an element of the isentropic line upon the v, T plane makes a constant angle with the projected line of greatest slope at the same point.

From equations (9) and (11) it follows that

$$\tan i'=\frac{k-1}{\tan i}; \quad\dots\dots\dots\dots\dots\dots \quad (12)$$

from which it will appear that for either very high or very low pressure the isentropic line runs at right angles to the direction of greatest slope. The condition that it shall coincide with the direction of greatest slope is

$$\tan i=\sqrt{k-1}=\frac{R}{p},$$

or

$$p'=\frac{R}{\sqrt{k-1}}. \quad\dots\dots\dots\dots\dots\dots\dots \quad (13)$$

For air this pressure is about 3·2 millimetres of mercury; and for other gases it is proportional to the volume of a unit mass at a standard temperature and pressure.

The thermodynamic surfaces of various gases will lie the one above the other, those having the largest value of R being uppermost. If we now substitute the value of p' of (13) in the original equation of the surface, we have

$$v=\sqrt{k-1}\,T, \quad\dots\dots\dots\dots\dots\dots \quad (14)$$

which is independent of R. Hence, for all gases which follow the law represented in (1), the lines on their respective surfaces, where the isentropic lines coincide with the direction of maximum slope (13), will all lie in a common plane passing through the axis of P and at right angles to the plane of v, T, its trace upon the latter plane being represented by (14).

If the gases have a common temperature while in this condition, (14) shows that they will also have a common density, which, when T is 273°, will be 0·000058 gramme to the cubic centimetre.

It will be observed that for air the pressure indicated in (13) is practically the same as that at which Maxwell's law for viscosity begins to fail. This, however, is a mere coincidence. The two phenomena have nothing in common, as is evident both from theoretical considerations and from experimental results.—Silliman's *American Journal*, August 1882.

ON THE INFLUENCE OF THE QUANTITY OF GAS DISSOLVED IN A LIQUID UPON ITS SURFACE-TENSION. BY S. WROBLEWSKI.

It has long been known that the solution in a liquid of a gas which is superposed to it diminishes the tension of its surface. M. E. Desains observed, twenty-five years since, that the rise of the meniscus terminating water at its contact with air was a little less than that formed with hydrogen, and a little greater than that formed with carbonic acid—that is to say, that it was lowered in proportion as the gas was more soluble. I have found that, in all the liquids which I have studied, the surface-tension in contact with air is a little greater than in contact with carbonic acid. Lastly, M. Quincke has shown that, in the case of ammonia and hydrochloric acid (which are highly soluble in water), the diminution of the surface-tension increased with the quantity of gas dissolved. On the other hand, a number of cases can be cited in which the more or less complete absence of the faculty of absorbing gases is always accompanied in a liquid by relatively great surface-tension, and conversely. Thus liquids whose coefficient of absorption is considerable (ether, alcohol, the oils) have a feeble surface-tension. Saline solutions, which absorb much smaller quantities of gas than water, have a greater surface-tension than the latter; and their tension increases with the quantity of the salt dissolved, while their capability of absorbing gases diminishes. By reducing the surface-tension of water by the addition of alcohol, the solubility of gases in the mixture thus formed is increased. And mercury, which, of all liquids, has the greatest superficial tension, is almost incapable of absorbing gas.

The study of the correlation between these two classes of phenomena may be commenced, on the one hand, by determining the laws governing the solubility of a gas in a liquid, and, on the other, by measuring the surface-tension of a liquid in contact with a gas, the solubility of which in the surface-layer of the liquid can be regulated at pleasure by increasing or diminishing the pressure upon the gas. These experiments are so much the more easy to perform, as the saturation of the surface-layer of the liquid is effected instantaneously, and the tension, depending only on the condition of that layer, follows with the same velocity every change in the pressure which determines the value of that solubility.

Up to the present time no one has considered the question in this light. On the contrary, it has been attempted to establish a theory of liquids which, while based on the facts which militate in favour of the existence of that correlation, denies the correlation itself. That theory, attributing to pressure a direct influence upon the surface-tension, leads to consequences at variance with the facts. Thus Kundt, having observed that the height to which a liquid ascends in a capillary tube diminishes in the same proportion as the pressure upon the gas is increased, has drawn from this fact the following consequences:—

There is an influence of pressure upon surface-tension. The observed diminution ought to be regarded as a tendency of the liquid to pass into the gaseous state. If the compression could be

carried far enough, not only all liquids, but solids also, such as salts, would finally be reduced at ordinary temperature to the gaseous state, as takes place under the action of heat. Finally, at a little higher temperature (M. Kundt made his experiments at one temperature only), cohesion being diminished by the increase of temperature, the decrease of surface-tension under the influence of pressure would take place still more quickly.

Having recently determined the solubility of carbonic acid in water under pressures of from 1 to 30 atmospheres, I proposed to myself to make evident and establish the correlation of these two classes of phenomena. Reserving the description of the method employed and the numerical data for a special memoir, I will only enunciate here the results of my experiments.

Under pressures of 1–30 *atmospheres, there exists a remarkable relation between the laws of the solubility of carbonic acid in water and the surface-tension of that liquid.* That relation can be expressed thus:—

1. *The product obtained by multiplying the surface-tension* α *by the pressure* P *under which the carbonic acid is placed is proportional to the saturation-coefficient* S *corresponding to that pressure*—that is,

$$\alpha P = AS,$$

where A is a coefficient which depends on the temperature and increases with it.

According to the first law of the solubility, the temperature remaining constant, $\frac{S}{P}$ *decreases in proportion as the pressure increases*. Experiment shows that the decrease of* α *is proportional to that of* $\frac{S}{P}$. With the aid of this relation of the phenomena of capillarity, those of the solubility of the gas can be calculated, and conversely.

2. *The pressure remaining constant and equal to* n *atmospheres* (n *being greater than* 1), *it follows from the laws of solubility that the quotient* $\frac{\left(\frac{S}{P}\right)_{P=n}}{\left(\frac{S}{P}\right)_{P=1}}$ *decreases with the lowering of the temperature. Experiment shows that in this case the ratio* $\frac{\alpha_{P=n}}{\alpha_{P=1}}$ *of the tensions corresponding to these pressures decreases also.*

This result is in evident contradiction of M. Kundt's theory, since the lowering of the temperature, instead of retarding the decrease of the surface-tension, accelerates it. The phenomena are therefore completely independent of the pressure, and depend only on the state of saturation of the surface of the liquid—that is to say, on the quantity of gas dissolved in the surface-layer.

The above relation does not end at the pressure of 30 atmospheres. The solubility increasing less quickly than the pressure, tends towards a certain limit, which at 0° seems to be reached at

* See my Note, *Comptes Rendus,* t. xciv. p. 1355.

the moment of the liquefaction of carbonic acid, since that liquid does not mix with water. Experiment shows that the decrease of the surface-tension, becoming slower with the increase of pressure, tends also towards a certain limit, which at 0° is reached under the pressure at which the liquefaction of carbonic acid takes place ; at that instant the surface-tension of the water is reduced to about one half *.

Bisulphide of carbon, which also does not mix with liquefied carbonic acid, behaves in a similar manner in contact with that gas. The decrease of its surface-tension also takes place at 0° much more quickly than at a higher temperature. It becomes slower, and ceases under the pressure of liquefaction of the gas.

In my next Note I will show that the phenomena present themselves under a different form as soon as we have to do with a liquid that mixes in all proportions with liquefied carbonic acid.—*Comptes Rendus de l'Académie des Sciences*, Aug. 7, 1882, t. xcv. pp. 284–287.

ON THE STRUCTURE AND MOVEMENT OF GLACIERS. BY M. F.-A. FOREL.

M. F.-A. Forel, of Morges, Switzerland, has recently published (*Bibl. Univ.* III., vii. p. 329) an important memoir upon glaciers, embodying the results of observations by himself and M. Ed. Hagenbach-Bischoff, with a discussion of these results and also of those obtained by other observers. His argument rests plainly upon the well-attested fact that glacial ice has a distinctly crystalline granular structure, the mass being composed of a confused agglomeration of individual crystals, each optically distinct—and, moreover, that the size of these crystalline grains increases from the upper margin of the glacier at the limit of the névé, where they have the size of a hazel-nut, down to the middle part, where the size is that of a walnut, and further down to the extremity, where they are as large as a hen's egg. For example, at the lower extremity of the Aletsch glacier, or that of the Rhone, the grains have a diameter of 7 or 8 centim. In regard to this gradual increase in size of the individual crystals, the author remarks that two suppositions are possible : either the growth of some grains must go on at the expense of others less favourably situated, one gaining what the next loses, and absorbing as much heat as is disengaged by the crystallization ; or each grain increases in size by means of the water which reaches it from above from the surface of the glacier. Of these two hypotheses, the first is rejected, on the ground that, wherever observations have been made, they have shown the grains to be all of sensibly the same size in the same region, and not to be some small, others large, as this explanation would require.

Accepting provisionally the second hypothesis, the author remarks that for the increase in volume of the crystals there are needed water, cold, and favourable conditions. About the last point nothing is definitely known ; but the others admit of further dis-

* The case in which one of these two liquids is superposed to the other does not come within the scope of this communication.

cussion. The water is believed to be afforded by the melting of the upper surface of the glacier under the influence of the heat of summer. This water runs over the surface of the ice, descends into the crevasses, and, if it be admitted that the ice contains capillary fissures (a point which is discussed later), much of it would be absorbed by the mass of the glacier and used in increasing the size of the crystalline grains ; the rest of the water flows off in the subglacial torrent. The low temperature needed for the solidification of the absorbed water is believed to be due to the continued loss of heat during the winter, the glacier as a whole being a mass the temperature of which can never be above zero, but may fall considerably below. The question as to the mean temperature of the ice at different seasons of the year is discussed at length ; and the author concludes, for a variety of reasons which cannot be quoted here, that the middle of the mass of the glacier has probably a temperature at the end of the winter several degrees below 0° C. This excess of cold would be partially expended in causing the solidification of the water which, as already stated, is absorbed into its mass and thus goes to increase its volume. The crystalline grains are therefore to be conceived as growing by accretion, successive layers being added to them at the expense of the water derived from surface-melting, and in the process of the warming of the glacier which goes on during the summer.

Assuming the correctness of the results of Hugi as to the increase in size of the crystalline grains—that is, in brief, that they increase from a diameter of 1 to one of 4 centim.,—taking 100 years for the time of their development, the author finds that the annual increase in volume is $4\frac{1}{2}$ per cent. Assuming, further, that the cold of winter is all employed in bringing about this increase, it is calculated that the hypothesis advanced is satisfied if the temperature of the glacier descends in winter to $-6°{\cdot}8$ C., or in round numbers $-7°$ C. This temperature, the correctness of which is obviously dependent upon the accuracy of the assumed data as to the rate of increase of volume, is too low to be accepted, and leads to the inference that a part of the increase is accomplished by a process different from that which has been described. Thus at the end of the summer a considerable portion of the glacier must be at the temperature of melting ice, and in the capillary fissures between the crystalline grains there must be water ; now, as the glacier cools down in the autumn, the first effect of the loss of heat would be the solidification of this water, and the consequent increase in size of the crystalline grains. Taking into account this last point, the author considers that the temperature that would have to be assumed for the glacier at the end of the winter would be quite within the range of possibility.

The hypothesis which has been advanced depends upon the assumption that the water can find its way into the interior of the glacial mass through the capillary fissures separating the individual grains. This point is one which is yet somewhat doubtful; and the author, after considering the various observations of Agassiz and others, which tell for and against the possibility of such a

penetration of the water, discusses the question from a more theoretical standpoint, and concludes that the assumption of the impermeability of the glacier is contrary to fact. He promises, further, to make this a special subject of observation at a later period.

In regard to the cause of the movement of glaciers, M. Forel places himself on the side of Hugi and Grad in supporting the theory of expansion, although modifying somewhat their hypothesis. On the old dilatation theory, it was the expansion of the water contained in the capillary fissures at the moment of their solidification to which the glacial movement was supposed to be due. According to the view of M. Forel, however, this special expansion plays a subordinate part; and it is rather the gradual increase in volume of the crystalline grain, due to the molecular affinity which causes a crystal to grow in the mother-liquor in which it is placed.

In discussing further the application of the hypothesis, a distinction is made as to the course of events during the youth and during the old age of the glacier. The glacier may be divided into three parts. The first is in the elevated region where the glacier has its commencement, that of the névé. Here the heat of summer is not sufficient to melt the whole volume of the snow which falls during the year; only a part of the snow is consequently transformed into water; and this penetrates into the layers below, and is solidified there: the temperature is much below the freezing-point. This is the region of the infancy of the glacier. Following this comes the line of separation, where the heat of summer is just sufficient to melt the winter's snow, and there is no excess of heat to attack the ice.

The second stage (that of the youth of the glacier) is found below this line of separation, where the summer's heat not only melts the snow but also partially melts the ice; the water so formed is absorbed and assimilated by the ice; and the temperature below the surface is, even at the end of summer, below zero. In this region the glacier is increasing in volume, and consequently moving downward. Then follows a second line of separation, where the water absorbed is all used in the increase of volume of the glacial grain. At this point the subglacial torrent has its origin; and at the summer's end the temperature is at 0°.

The third stage is that of the old age of the glacier, where the supply of water exceeds that needed to bring the temperature of the ice back to 0°; the excess of water flows off in the glacial streams. The temperature of the ice is at 0° during the summer; and the excess of the summer's heat goes to cause the melting and destruction of the glacier.

In concluding his interesting memoir, the author promises to test his hypothesis by further observations and experiments, bearing especially upon the questions as to the comparative size of the crystalline grains in the different parts of a glacier, and as to the possibility of the penetration of the surface-water into the mass of the ice.—Silliman's *American Journal*, August 1882.

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XXIX. *Notes on Practical Electricity. By* R. H. M. Bosanquet, *Fellow of St. John's College, Oxford.*

THE work I have had in hand for some time past has been the design and construction of clock-regulated uniform-motion machines. As to these, I will only say that the constructions formerly described by me have been entirely superseded by a new design. This is not yet in a state of sufficient forwardness for description, and I propose to reserve it for another occasion. The working of these machines is dependent on the application of a considerable amount of electrical power. After seeing the Paris exhibition I decided that in all probability a dynamo machine would be a better source for laboratory purposes than batteries, which are in my opinion a nuisance, and subject to most serious defects when used on the large scale. I accordingly set up an A Gramme machine. I also have made a set of accumulators. The practice of the employment of these instruments for ordinary purposes gives rise to numerous points of interest; and I propose to give a short account of my experience.

The laboratory steam-engine is able to supply sufficient power to give a powerful electric light from the machine. I take it that under these circumstances about $2\frac{1}{2}$ horse-power are absorbed altogether. The nominal horse-power of the engine is 2. It is scarcely sufficient to develop the full power of which the dynamo is capable; but for laboratory purposes it is quite sufficient.

The A Gramme machine has four electromagnets, two above and two below, whose axes are in one plane. In the same plane is the axis of the Gramme ring armature. The resistance of the machine is about 1·2 ohm.

The magnet-wires are directly connected with the armature-brushes; and the terminals receive the wires after each has passed through two magnets. Consequently all currents have to pass through both magnets and armature, thus:—

```
Magnet———Armature———Magnet
  |                   |
Magnet              Magnet
  |                   |
Terminal            Terminal
```

For purposes which will be presently described, it was found necessary to be able to separate the magnets from the armature. The wires were therefore cut, by which means the following combinations were obtained :—

```
 ┌— Magnet-terminals —┐
 |                    |
Magnets—┬Armature┬—Magnets
        |        |
   Armature-terminals
```

and

```
┌— Magnet-terminals —┐
|                    |
└——— Magnets ————————┘
┌——— Armature ———————┐
|                    |
└— Armature-terminals —┘
```

in which last arrangement the magnets are separately excited. There are other forms, which will be described presently.

The resistance of the magnet circuit is about ·75 ohm. That of the double course through the armature is therefore about ·45 ohm, the total resistance being taken at 1·2.

The terminals are marked + and − respectively. When the machine is acting in correspondence with these indications, I call it " straight," and the upper pole-piece attracts the unmarked end of a compass-needle. The residual magnetism is very strong. I think the whole of the solid part is probably made of cast iron.

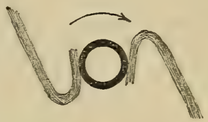
When the polarity gets reversed from any cause, I call it "reversed." In this case the upper pole-piece attracts the marked end of a compass-needle.

The directions of currents and rotation deserve some attention.

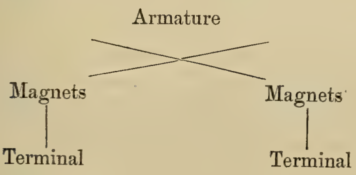
The machine is driven in the direction opposite to that of the hands of a watch, regarded from the pulley-end. It gives currents in the one or the other direction according as it is "straight" or "reversed." This is obvious; for with reversal of the current both field-magnets and armature are reversed, and the attractions which have to be overcome in doing the work remain the same.

When a current is sent through the machine from a battery, it always turns in the direction opposite to that in which it is driven by the engine; for whichever way the current goes the attractions are the same as in the former case, and as these are overcome by the engine in that case, it is clear that they tend to turn the machine in the opposite direction to the engine.

When the machine is driven by a battery, with the hands of a watch from pulley-end, it is necessary so to adjust the brushes that their ends may not catch in the commutator. I turn up the ends of a spare pair of brushes, and apply them to the commutator in this manner.



Otherwise the machine can be caused to turn in its normal direction when driven by a current, by reversing the connexions between armature and magnets; this is made possible by the cutting of the wire above described. The arrangement may be formulated thus:—



The machine is constructed for driving one arc light. Although this is not a matter with which I am directly concerned, the normal conditions may be of interest:—

Serrin lamp.
Revolutions about 900 per minute.
Current about 20 ampères.
Difference of tension between lamp-terminals about 40 volts.

These are the highest values obtained under favourable circumstances.

According to these numbers the resistance of the arc is about 2 ohms; that of the machine and connexions is about 1·5.

This corresponds to 800 volt-ampères, or rather more than 1 horse-power, in the light itself (1 horse-power = 746 volt-ampères).

We observe here that little more than half the power expended is developed in the lamp.

Circuits of High Resistance.

Suppose I take a single British incandescent lamp (150 ohms cold, and about 80 ohms hot), the machine only just raises it to a red glow, though a speed of as much as 2000 revolutions per minute be employed. The current developed is much less than 1 ampère.

Here the high resistance kills down the current, and prevents the proper excitation of the field-magnets. Hence the advantage in this case of machines in which the magnets form a shunt circuit of high resistance. It is easy to excite the Gramme machine by putting the magnets into a shunt circuit with a suitable resistance; but if a resistance-wire be employed, the power developed in heating it is wasted. I have therefore in many cases adopted the plan of putting useful work into the shunt-circuit of the magnets. This may sometimes with advantage consist of a number of incandescent lamps in parallel circuit. Substituting for this a certain number of accumulator-cells, we have the origin of a useful method which I call charging in balanced circuit, to which I shall return.

Galvanometers.

The galvanometers I employ were made in the laboratory. They are of a simple character, but quite sufficient for practical purposes. There are three of them.

Tension Galvanometer.—A circular wooden channel about ·11 m. radius, wound with wire of which 1 metre = roughly 10 ohms. Resistance = 2900 ohms. This had its constant determined by reading the current from 9 Daniell's cells, which are taken as representing 10 volts. Resistance of Daniell's cells determined and allowed for. For high ten-

sions a further resistance of 5000 ohms is interpolated in the galvanometer circuit. Brass dial 3 inches in diameter divided to degrees. Short steel needle with long pin-points. 50° on the galvanometer, with the 5000 ohms external resistance, corresponds very nearly to 50 volts. Other readings by the tangent-law.

Two Quantity Galvanometers.—These consist each of a ring of gun-metal, ·11 m. radius; the conductors leading to them are copper, and of tube-and-core form; brass dials, and steel needles with long pin-points as before.

When dealing with large currents I placed a steel magnet under one of these, so as to convert it into a high-quantity instrument. Its constant was ascertained by sending the same current through both instruments, the one without the magnet reading as a tangent galvanometer in absolute measure.

Measures.

Although numerous measures of dynamo machines have been published, the laws of any given machine cannot as yet be predicted; and a few measures are given, which are sufficient to illustrate the general course of the performance of this machine under different circumstances.

First, we will consider the cases where the governor was used. This may be taken to give 840 revolutions in all cases, except where, the resistance being 3 ohms or less, the leverage against the engine was such as to reduce the speed materially.

Resistance in parallel circuit with lamp. ohms.	Tension between terminals. volts.	Quantity. ampères.
7	24·5	3·8
6	31·4	5·5
5	37·9	7·6
4	41·0	10·5

With lower resistance the speed cannot be maintained without taking the governor off. In considering the total electromotive force developed, of course the whole resistance of the circuit must be considered. The resistance of machine and connexions may be taken at 1·5. Then, multiplying the current by the whole resistance, we should have the total electromotive force.

The work of Meyer and Auerbach leads for a given speed to equations of the form $\theta = a\,\mathrm{E} - b$, where θ is an angle whose tangent is the measure of the current, and E the total electromotive force. I used the expression at first as it stands;

but now I think it better to eliminate E as follows:—Putting $E = CR$, the above form gives $CR = \frac{\theta + b}{a}$ or $R = \frac{\theta + b}{a\,C}$, where R is the resistance of the circuit, C the current, and θ the angle of a tangent-galvanometer by which the current is measured.

To apply this to the above observations, we find the following values of the total resistance, current, and θ:—

R.	C.	θ.	$R = \frac{\theta - 20.}{C}$
7·9	3·8	50°	7·9
7·0	5·5	60	7·3
6·2	7·6	67½	6·2
5·3	10·5	73	5·1

(When calculating the resultant resistance of the parallel circuit, we notice that it should be the same as the quotient of the observed tension by the current. It does not differ in any case by more than one or two tenths of an ohm. This is sufficient, considering that no special accuracy was aimed at. Further, it must be remembered that the wires of the resistance are nearly or quite red-hot under these circumstances, so that some discrepancy is to be expected.)

Calculating R from the formula

$$R = \frac{\theta - 20}{C},$$

we obtain the numbers in the last column.

This result is tolerably satisfactory; but it is of little practical use where the motor employed is a steam-engine capable of considerable variations of speed. With a gas-engine, or other motor of very constant speed, the above method of finding the approximate resistance for a given current would probably be useful.

But with my engine, when the governor is not used, the speed adapts itself to the work to be performed; so that, within considerable limits, the only things to be considered are the amount of steam turned on and the work to be done. To illustrate this I will quote a series of experiments, in which the object was to maintain the tension necessary for a "British" incandescent lamp, which is about 80 volts, with different resistances in parallel circuit.

The least resistance with which this tension can be maintained is 5 ohms. It is easily seen that the resulting external resistance here is nearly four times that of machine and connexions; so that the total E.M.F. would require to be 100 volts, roughly, to get 80 between the terminals. For smaller

external resistances the total required would be larger; and this cannot be obtained. The load on the engine is heavy; full steam is required; and the speed is moderate.

Increase the 5 ohms to 6, leaving the steam as before. The engine runs faster, and the required difference of potential is kept up with about the same head of steam.

Increase to 7 ohms. Similar effect.

Increase to 10 ohms. The engine runs very fast; but the required difference of potential is still easily maintained. Of course here the total E.M.F. required is decidedly lessened.

As the resistance increases beyond this point, the speed required to maintain a difference of potential of 80 volts becomes too great for convenience, and by the time it reaches 20 or 30 ohms it becomes impossible.

In view of this power of accommodation, the question as to the precise resistance required to produce a given current at a given speed becomes of little practical importance.

The explanation of the power of accommodation is, roughly, as follows. The total E.M.F. is to be supposed constant. In the preceding experiments it is not quite so, being 100 volts, about, with the 5 ohms, and 90 or so with the 10. But the conditions of constancy can be realized; for with the 10 ohms I kept up the difference of potential of the terminals to 90 volts for some time, this corresponding to about 100 volts total E.M.F.; so that, in fact, this condition of constancy of total electromotive force can be attained through a certain range with a little attention.

Then the current developed follows Ohm's law throughout this limited range, with varying resistances; and the attraction between the magnets and armature depends on the current in each; so that a rough idea of the course of the values may be formed by assuming the attraction proportional to the square of the common current through both; and this is the reaction against the engine. Consequently as the resistance in circuit increases the current diminishes (Ohm's law), and the statical resistance to the engine diminishes, as we suppose for the moment, in the square of the ratio of increase of resistance in the circuit. Of course, the real values of the attraction depend on the magnetizations of magnets and armature; but the above accounts in a general way for the increase of speed produced by introducing additional resistances into the circuit. The increased speed is then utilized in obtaining the same electromotive force from the diminished current, or rather from the diminished magnetization.

It will be readily seen how importantly this power of accommodation may be utilized in the practical treatment of such

a question as the variation of the number of incandescent lamps on a circuit. Take the case of lamps of 70 ohms. Then 7 of these in parallel circuit constitute a resistance of 10 ohms, and 14 one of 5 ohms. Between these limits the tension might be kept nearly constant by the automatic variation of speed above mentioned. I do not say that this is the best way of attaining the result; but with means such as I possess it is a valuable auxiliary. I believe as many as 25 such lamps can be driven from the A machine; but this must require far greater power than is at my disposal.

Accumulators.

I have made 21 cells with lead plates coated with red lead, and 9 of another kind, which I will describe presently. In all cases the plates are wrapped in canvas. The lead I employed was 2 lb. to the square foot; each plate is about a square foot in size; and the red-lead plates have 1 lb. of red lead on each surface. The tags at the end were left rather narrow. I found lately that all the tags of the oxidized plates were so eaten away at the surface of the acid that they had to be replaced. I now use lead 3 lb. to the square foot for oxidized plates; the tags are left 3 or 4 inches broad, and they are additionally thickened at the junctions by burning on a piece of lead on each side; they are also varnished.

These cells were "formed" by charging in the same direction every day for a considerable time. The maximum efficiency was reached in about a month, after which no further improvement was perceived.

These cells do not pretend to the excellence which is said to be obtained by the cells of the Faure Company. If left charged for any time, the sulphuric acid is rapidly and completely absorbed, leaving the water quite sweet, and considerable quantities of sulphate of lead are formed. This is slowly decomposed by fresh charging; but in the mean time it appears to isolate large portions of the active material, and considerably impairs the efficiency. I am informed that this local action does not take place with the cells supplied by the Faure Company. The red lead on the hydrogen plates becomes reduced to metallic lead, and that of the oxidized plates changed into black peroxide *. It is not until this change is complete that the cells attain any considerable efficiency.

* I am indebted to Mr. Fisher, of the Oxford-University chemical laboratory, for the examination of samples of these substances. The metallic character of the reduced substance was proved by amalgamation with mercury. The peroxide contained very nearly the proper quantity of oxygen, but fell a little short—not more than would be explained by inevitable impurity.

The cost of the construction is very considerable. I have little doubt that they have cost me on the whole as much as if I had bought them at the very high price charged. But much of the expense could have been avoided. As it was, the original construction cost under £2 per cell. About half the outlay went to provide the wooden cases lined with lead. If stoneware jars could have been procured, I think one third of the original cost might have been saved. The "forming" of the cells with the small power at my disposal was expensive. I have not estimated the cost; no doubt it could be done very much more cheaply on the large scale.

The best return I have ever obtained amounted to about one half the power expended. This was obtained after a charge of short duration, the cells running one Swan lamp. The current and tension were measured at intervals. The return was about 70 per cent. of the electrical charge taken, and, as I judge, about half the total power expended. But this is by far the best return I have ever obtained. As a rule, the return is only a small fraction of the power expended.

The other 9 cells are made with amalgamated plates of lead, and "formed" by charging in opposite directions in the manner practised with Planté batteries. These have less capacity than the red-lead cells, and are troublesome to form; but on the whole I am inclined to prefer them for laboratory purposes to home-made red-lead cells. They are much less trouble to put together, and do not suffer from local action.

Having cells of different kinds in the system is a great inconvenience. When the red-lead cells have suffered from local action, they increase their resistance; and if systems of balanced charging are employed, in which two or more circuits are used, a progressive change takes place in the distribution of currents, which requires constant watching. It should be a first principle that all the cells should be as similar as possible.

The red-lead cells have 11 plates in each cell, the others 15.

With all their imperfections these cells are extremely useful for laboratory purposes; and as this was my object in constructing them, I am on the whole satisfied with their performance.

On charging Accumulators.

Suppose that the accumulators are entirely without charge, and the machine polarized rightly, having its residual magnetism such that the + terminal gives a positive current and the − one a negative current. Then in charging we join + (or oxygen) to +, and − (or hydrogen) to −, and all goes

rightly. Now suppose that, after the accumulator has acquired a sensible charge, we stop the dynamo machine without breaking the connexions. The power of the machine to drive back the battery-current was derived from its velocity, which is gone. Consequently the battery-current discharges itself through the machine in the opposite direction to the proper current of the machine, and reverses the magnetism of the machine. This change is recognized, as I have mentioned, by the effects of the pole-pieces on a pocket-compass. When the machine is "straight," the upper pole-piece attracts the unmarked end of the needle. When it is reversed, it attracts the marked end.

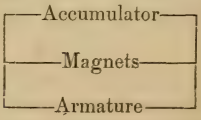
If the machine be then set in motion again, the effect is to strengthen the current in the magnets. But this is now the discharge current of the battery. Consequently the machine proceeds to pump the electricity out of the battery.

This reversal is prevented in ordinary practice in two different ways. In charging by Gramme machines, in general, two machines are employed, of which the one serves only to drive a current through the magnets of the other. This current is wholly independent of the charging current; and the arrangement is consequently not liable to reversal.

The power which is employed in this case to maintain the current in the magnets contributes nothing to the work; consequently this arrangement is not economical. But in a large establishment one small machine may be used to excite several large ones, and the waste is reduced to a minimum.

The best method for charging in general would appear to be the use of machines of the Siemens type, with the field-magnets in a shunt circuit of high resistance; for then the reaction of the battery-current seeking to pass backwards into the machine conspires with the forward current from the machine, so far as the supply of the field-magnets is concerned.

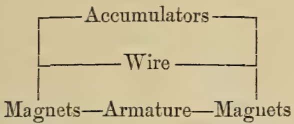
Diagram of Circuits in Shunt-Circuit Machine.



But as my machine is not thus arranged, I have had to devise other means of doing the work. I have not seen any account of these processes.

The simplest mode of effecting the charge is to charge in one series, starting by means of a resistance-wire in parallel circuit, thus :—A wire of 5 or 6 ohms resistance, arranged so that it can become red-hot without injury, is introduced between the terminals, and the machine driven at full speed. A difference of tension is thus produced between the ends of the wire, which must be greater than that of the accumulators to be charged. This difference amounts with my machine to 70 or 80 volts, or even more. Then, if we have 80 volts and 6 ohms of wire, we get a current of a little over 13 ampères, which is enough to excite the machine well.

The terminals of the battery are now joined up to the terminals between which the difference of tension is established, + to + if the machine is straight, + to − if reversed. The existing difference of tension drives back the battery-current, and a charging-current is set up. With the 30 cells in series I find that the current traversing the accumulators is about 10 ampères under these circumstances.



The machine is then traversed by the double current of more than 20 ampères from both battery and resistance-wire. Great statical resistance to the engine is consequently developed, and the magnets are well excited; but the current through the 6-ohms wire goes to waste, and in fact the wire is kept nearly red-hot. A large power is required to maintain this state of things. The last step is to remove the wire, leaving the battery with the current of about 10 ampères passing through it. The resistance at the machine is reduced to about one fourth by the halving of the current, and the engine quickens its speed; but, on the other hand, in order to maintain the difference of over 70 volts when excited only by 10 ampères, the machine requires a high speed.

Consequently there is a period of instability at the moment of removing the resistance, and the machine is often reversed before the high speed is established. The only means of obviating this are (1) to cram on every available pound of steam at the moment, (2) to execute the two operations of joining up the battery and removing the resistance in such quick succession that the speed has not time to fall. By the use of both these precautions I have generally succeeded in

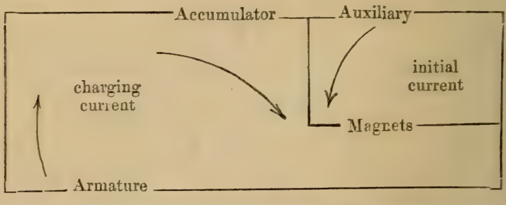
starting the charging in this form; but until I had acquired considerable experience and a thorough understanding of the conditions, I frequently failed.

When this arrangement is once started, and if sufficient power is available to keep it going, it is not only the most economical method of charging with this machine, but is more economical than any other method, excepting the case where the magnets form a shunt circuit of very high resistance indeed.

The objection to the arrangement in question is that there is a very small margin between the tension of the battery and that of the machine which has to overcome it; consequently at the slightest check to the steam, and frequently without any apparent cause, the battery-current will overpower the tension of the machine and reverse it. The motion of the machine tends to reinforce the current of the battery in the new condition, and the resistance of the machine is small; so that a tremendous current, probably more than 50 ampères, is poured through the machine, and the electromagnetic attraction resulting is powerful enough to stop the engine. This reversing is indicated by a hiss of the belt on the pulley, followed by the stoppage of the engine. The person in attendance has immediately to break the circuit, or the whole store of electricity would soon be poured uselessly away.

With the limited power at my disposal I have never been safe from reversals in charging in series in this manner when the number of cells to be charged amounted to twenty or more.

Another method of starting the charging in series is to throw a current through the magnets from an auxiliary battery or from a series of accumulators already charged, and then introduce those to be charged. The connexions are as follows:—



In this arrangement one terminal of the magnets must be disconnected from the armature, and the magnets made up into one circuit.

When the charging is started, the auxiliary battery is removed. An instability of the same sort as that above described takes place.

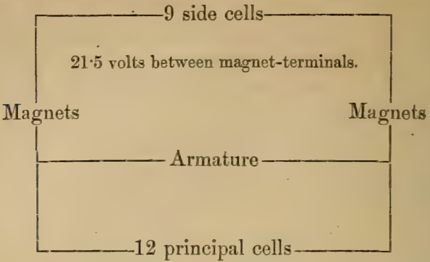
This method is not superior to the last, and is troublesome from requiring the auxiliary battery.

A very simple mode of charging, where the cells are all similar, is to make them up into two or more parallel circuits, according to the electromotive force available. When there are different kinds of cells this method is troublesome, as the two circuits have to be exactly balanced, and this must be done by trial. Further, the red-lead cells alter during charging as if their resistance diminished, which I take to be caused by the decomposition of the sulphate of lead with evolution of sulphuric acid. It is then necessary to keep watch at the galvanometers in the two parallel circuits and rearrange the circuits when necessary, which is very troublesome. Latterly, however, I have used this method more. With my machine it would be more suitable for charging about 40 cells. The charging is started with a resistance in the same way as in the first case.

The next method is an interesting one; I found it useful when the cells were in good condition, with low resistance and with about 20 cells; but I have used it also with 30 cells. When the resistance of the cells is high, it is better to use the last method with 30 cells.

I call the present method the method of balanced charging. It arose in this way:—I began by putting the magnets in a shunt circuit, with a resistance in the magnet circuit; then accumulators could be charged in the main circuit (armature-terminals), and the current spent in heating the resistance in the magnet circuit went to waste. I therefore tried substituting cells for the resistance in the magnet circuit. I put the two similar quantity galvanometers in the two circuits, and varied the distribution of the cells until the deflections were equal; so that all the cells were getting uniformly charged. I found at once that under certain conditions a balance was established, so that the tension-difference on the two sides of the magnet circuits was small, and that, so long as the balance was maintained, there was no tendency to reversal.

The following scheme will explain one of these arrangements:—



28·1 volts between armature-terminals.

In this case I had a balance when 9 ampères of current were passing in each circuit.

It is clear that the current through the magnets is due to the difference of tensions between the two sets of cells; consequently,

		volts.	
Tension of	12 cells	=28·1	(measured)
„	9 „	=21·5	„
		6·6	

and, dividing by the measured current of 9 ampères, we find ·73 ohm for the resistance of the magnets. This corresponds well with my direct measure of ·74, and less well with the resistance given by the makers, ·77.

The balance is a function of the current; and if the power in action be varied, the current in the principal cells changes faster than that in the magnet circuit (or side cells). It is to this circumstance that the stability of the arrangement is due.

In order to give foundation for the theory of this arrangement we have to represent the variation of the tension of the cells with varying current. We may represent the course of the changes sufficiently for this purpose, according to my measures, by assuming that the tension varies with the charging current, so that it is 1·9 volt per cell when at rest and 2·4 per cell when the current is 10 ampères, *i. e.* it varies $\frac{1}{20}$ volt per cell per ampère. The same rule expresses fairly well the diminution of tension when the freshly charged battery gives out a current. I shall return to the question of the rationale of this, and for the present assume general values, and that the change of tension is proportional to the current.

Let E be the tension of one cell at rest,
x the current in the principal cells,
y the current in the side cells,
ϵ the change of tension per cell per ampère,
m the number of the principal cells,
n the number of the side cells.

Then,

Tension of armature-terminals, or of principal cells
$$=m(\mathrm{E}+\epsilon x);$$

Tension of magnet-terminals, or of side cells
$$=n(\mathrm{E}+\epsilon y).$$

Then, if R be the resistance of the magnets, since the difference of these tensions drives current y through R,

$$m(\mathrm{E}+\epsilon x)-n(\mathrm{E}+\epsilon y)=\mathrm{R}y;$$

or

$$(m-n)\mathrm{E}+m\epsilon x=(n\epsilon+\mathrm{R})y;$$

$$\frac{dx}{dy}=\frac{n\epsilon+\mathrm{R}}{m\epsilon}.$$

Put $n=m-d$, where d is the difference between the principal cells and side cells; then

$$\frac{dx}{dy}=1+\frac{\mathrm{R}-\epsilon d}{m\epsilon};$$

which is >1 if $\mathrm{R}>\epsilon d$.

In the case above given,

$$\epsilon=\cdot 05, \quad \mathrm{R}=\cdot 74, \quad d=3;$$

whence $\frac{dx}{dy}=2$ nearly, and the change of the current through the principal cells is twice that in the magnet circuit.

Of course it is the change in the magnet-current that alters the excitation, and gives rise to instability; so the more we throw the change off the magnet-current, the more stable the arrangement will be.

The behaviour of the balance as ascertained by experiment corresponds with this theory. The magnet-current alters more slowly than the other.

To deduce the magnitude of the balancing current in any particular case, put $y=x$ in the equation

$$m(\mathrm{E}+\epsilon x)-n(\mathrm{E}+\epsilon y)=\mathrm{R}y;$$

$$(m-n)\mathrm{E}+\{(m-n)\epsilon-\mathrm{R}\}x=0.$$

Let $m-n=d$ as before,

$$x=\frac{\mathrm{E}}{\frac{\mathrm{R}}{d}-\epsilon}.$$

If $\mathrm{R}=\cdot 74$, $d=3$, $\epsilon=\cdot 05$,

$$x=5\mathrm{E}.$$

If we assume $\mathrm{E}=1\cdot 9$, $x=9\cdot 5$, which is near enough to 9, the observed value, to show that we have a general representation of the facts. The constants are not determined with sufficient closeness for accuracy.

Again, suppose we wish to charge with a larger current. Put $d=4$ in the above; then we find for x the balancing current 14·6. So that we have only to increase the difference between the two sets of cells.

Again, supposing we wish to use a smaller current, put $d=2$. Then $x=6$, nearly; or the balancing current is about 6 ampères. Both of these conclusions have been verified experimentally.

With reference to the assumption just made as to the dependence of the potential on the current, and generally as to the constants of the accumulator, if we calculate the resistance of a cell of the accumulator from the data available, assuming that the plates are 1 centim. apart, and that the sulphuric acid has a strength even considerably less than 10 per cent., which is its original value, we find a result which is extremely small. In fact, specific resistance *

$$\begin{array}{lll} \cdot 2 & \text{per cent.} & =4\cdot 47\times 10^{10}, \\ 8\cdot 3 & \quad ,, & =3\cdot 32\times 10^{9}. \end{array}$$

The intermediate value 10^{10} represents our case with sufficient approximation.

The edge of each plate is about 30 centim., and its surface not far from 1000 square centim.; so we may consider the conducting solid as consisting of one plate of unit thickness, and surface somewhat exceeding 10,000.

The resistance therefore would be $\frac{10^{10}}{10^4}=10^6$, or the thousandth of an ohm roughly. This prevents us from assuming that the change of potential caused by current is due to the spread of the tensions along a resistance according to Ohm's law. And it appears probable that any change of tension due

* These numbers are taken from Prof. Everett's 'Illustrations of the C.G.S. System.'

to such a source is negligible, so long as there is acid in the solution.

A key to the nature of the phenomenon may be found by considering what happens when the cells are being exhausted. If a current of 10 ampères, such as I have used for charging, be demanded from the accumulators, the tension falls off. If the demand continues, the cell becomes exhausted. If we let it stand, it will recover itself, just as is the case with a Leclanché cell, for instance*.

Now the simplest way of accounting for this, as well as for the rise of tension in charging, is to suppose that the chemical action, in which the storage consists, does not actually reside on the surface, but penetrates to a certain extent into the subjacent layers of the mass. It is easily conceivable that such an action should be only capable of transference through the mass at a certain rate; that when too large a current is demanded from the cell, the chemical change does not return from the interior layers to the surface with sufficient rapidity to maintain the current; and that when, on the other hand, a high-charging current is employed, it cannot get away fast enough into the substance from the surface, and is accumulated there and forced to a higher intensity. It is simplest to assume that this heaping-up is directly proportional to the current which causes it.

So far as the excitation of the magnets goes, the method of balanced charging is more economical than charging in series; but as a double current passes through the armature, there is some considerable waste in heating it.

The electromagnetic attraction of the machine is in this case roughly double that of charging in series (assuming for the moment that the magnetization is proportional to the current). This gives the engine a better hold of its work in the case considered without making the load so very heavy as when the double current passes through both magnets and armature.

The lead of the dynamo machine, or the angle at which the brushes have to be set forward on the commutator, is about double what it is under other circumstances. This is no doubt due to the exalted magnetism of the armature due to the double current taking longer than usual to be affected by the comparatively weak field-magnets through which only a single current is passing. This appears to point to the reduction of the

* So, if we try to drive an arc light, we can get for a short time a current of more than 20 ampères and a good light; but the battery soon begins to be exhausted, and a most curious effect sets in: the alternations of exhaustion and recovery succeed each other with great rapidity, and the carbons begin to chatter in a most extraordinary way.

lead by the employment of more powerful field-magnets. In all cases the lead must be ascertained by its being the position of least sparking, and the brushes set pretty closely to it; otherwise the brushes are burned away, and the sparking is unpleasant.

Having charged the accumulators, I divided them up into circuits as required, and employ them to work my electro-pneumatic and other appliances. I have driven every thing for a week after a charge of a couple of hours; but on account of the local action in the red-lead cells, this is not a very advantageous course; and latterly I have preferred to charge for half an hour in the morning of each day. If the charge is meant to last, of course the currents must be used economically.

There are considerations, however, connected with the inevitable variation in the power of the current when any description of cells is used as a source of electricity, which make me already doubtful whether this alternate procedure will answer, and whether it may not be necessary to use the cells with the machine running simply for the purpose of sluicing off small currents with fairly constant differences of tension at their sources from the main current of the machine. It is even possible that simple pairs of lead plates in acid without preparation may be sufficient for this purpose. But the discussion of this matter must wait until my uniform rotation-machines are in a more complete state.

I will close with an experiment showing, in a manner suitable for the lecture-table, the development of a counter electromotive force in the dynamo when driven by a current from the accumulators.

I place a Swan lamp in parallel circuit with the machine. Then, so long as the machine is at rest, the lamp does not burn. But when the machine is in motion, as the velocity increases the counter electromotive force is set up, and the passage of the current through the machine more or less completely barred. The lamp in the parallel circuit then burns up. If the machine is stopped by a brake or overloading, the lamp is extinguished again.

XXX. *On the Methods employed for determining the Ohm.* *By* G. Wiedemann*.

THE Congress of Electricians which met at Paris in the autumn of last year adopted the electromagnetic units based on the centimetre-gramme-second system for the con-

* Translated from the *Elektrotechnische Zeitschrift*, July 1882. From a separate impression, communicated by the Author.

stants of the galvanic current, as fundamental units, and expressed a wish that a special international commission should be intrusted, in the first place, with the construction of a standard ohm as unit of resistance. Since, then, further consultation is to take place before very long, it seems desirable to consider again the methods hitherto employed, with their sources of error, from the experimental standpoint; their mathematical theory has been sufficiently discussed.

I desire also to give an impulse to further discussion of the methods to be adopted in the new determination. This is to be done the more strictly and thoroughly, since the units, having been once determined, ought not to be altered again immediately in consequence of further investigations.

It is well known that W. Weber, to whom we owe the fundamental facts in this subject, has given four methods of obtaining a conductor of given resistance in electromagnetic units:—

I. A circle of wire of known dimensions is caused to revolve through a certain angle about an axis (vertical) inclined to the direction of the earth's magnetism; and the intensity of the current thus induced is measured by means of a galvanometer of known dimensions. The intensity of the current, under similar conditions, is inversely proportional to the resistance of the conductor.

II. Instead of measuring the dimensions of the galvanometer in the first method, the action of the unit current on the magnetic needle of a multiplier is determined by the damping of its oscillations when the circuit is closed.

III. A magnetic needle is allowed to oscillate within a closed multiplier of known dimensions, and the damping of its oscillations is determined.

IV. A circle of wire is put into uniform rotation about a horizontal or vertical diameter; and we observe the deflection which takes place in a magnetic needle swinging at the centre of the circle, in consequence of the current induced in the revolving circle by the earth's magnetism. Since in carrying out these methods each separate measurement is of necessity attended with error, that method appears at the outset the most reliable which involves the determination of the fewest constants, and in which these determinations can be made with most accuracy. Hence methods III. and IV. appear at the outset to offer special advantages.

We will consider the fourth method first, in order to discuss at once a number of sources of error which partly affect also the other methods. This method is in fact that employed

by the Committee of the British Association appointed for the purpose in 1863.

A wire coil is put into rotation about a vertical axis, in consequence of which currents are induced in it, whose intensity in unit time is, on the one hand, directly proportional to the horizontal component of the earth's magnetism and to the change of the projection of the plane of the coil on the vertical plane at right angles to the direction of that component, and, on the other hand, inversely proportional to the resistance of the coil.

But, then, the induced currents which traverse the coils act upon the neighbouring coils, and induce in them extra currents whose electromotive force is proportional to the change in the directly induced current, in the unit time; hence the inductive action in the spiral is diminished. The deflection of the needle is determined by the total action of all the induced currents. The following investigations are therefore necessary to determine the absolute resistance of the coil.

1. The measurement of the space enclosed by the windings of the coil, to which the induction is, *cæteris paribus*, proportional, as well as its form, upon which the extra currents induced in it depend, and the moment of rotation exerted on the magnetic needle at its centre.

These determinations offer very considerable difficulty.

If thick wire is employed for winding the coil, its diameter, together with the insulating covering, must be exactly an aliquot part of the interior width of the frame on which the wire is to be wound, or else the outer layers of wire will be squeezed more or less between the wires of the inner layers, and so cause displacement. Moreover, as W. Siemens* has shown, the wire is stretched in winding, the more the thinner the wire is; and this extension may be as much as 6 per cent. Again, the insulating covering of the wire becomes pressed together; this takes place less when the covering consists of solid gutta-percha or similar substance than when the wire is covered with silk or cotton. The thinner the wire is, the more important do these errors become, of displacement of the wire, of extension on winding, and of the squeezing together of more of the insulating covering in proportion to the diameter of the wire. It is therefore not correct to calculate the space enclosed by the coils from the length of the wire before winding or after unwinding, and from the dimensions of the coil.

On this account, as W. Siemens rightly remarks, the accurate measurement of the length of the wire of about 1·1 milli-

* Poggendorff's *Annalen*, 1866, vol. cxxvii. p. 327.

metre thickness to the tenth of a millimetre, as in the older experiments of the British Association, was useless.

It is not difficult to determine the internal diameter, in various directions, of the coils of wire (that is, the diameter of the frame on which they are wound), either by means of a kathetometer, or with an inextensible steel tape of constant temperature; but, on the other hand, the measurement of the external diameter or circumference is much more difficult, in consequence of the inequality of the covering of the wire and the unevenness of the surface.

If the error in the determination of the mean diameter only amounted to 0·5 millim., which, in view of the circumstances mentioned, is certainly not an extreme estimate, then in the case of the coil of 314 millim. diameter employed by the Committee of the British Association the estimate of the space enclosed by the coils would be wrong by $\frac{1}{314}=0{\cdot}32$ per cent. In order to reduce this error as much as possible, it is necessary, as both W. Weber * and Lord Rayleigh † recommended, to take the diameter of the coil as great as possible consistently with accuracy of rotation. Also the wire should be wound with a tension as uniform as possible; and the exterior diameter should be controlled after each layer has been wound.

A much more important source of error lies in the uncertainty of determining the mutual position of the separate coils, depending upon the conditions explained above, upon which the intensity of the extra current induced in the coil when put into rotation depends. Since the inductive action of the coils upon each other takes place at very small distances, a very small error in measuring the distance apart is of great importance.

A further source of inaccuracy is introduced by making the coil of two parts parallel to each other, but with a space between in order to admit the thread by which the magnet is suspended; so that here also the parallelism and distance apart of the two portions must be very exactly determined. That the data may be greatly altered by the extra currents is seen from the fact that, in an experiment of a Committee of the British Association, the position of maximum induction of the coil in rapid rotation was displaced by not less than 20°; and the correction for this amounted to some 8 per cent.

If we seek to determine the self-induction by opposing the coil to another of known coefficient as a Wheatstone's bridge ‡,

* *Berichte der Königlich Sächsischen Gesellschaft der Wissenschaften*, 1880, p. 77.

† Proceedings of the Royal Society, 1881, vol. xxxii. p. 122.

‡ Compare Maxwell's 'Treatise,' vol. ii. p. 357; Brillouin, *Comptes Rendus*, vol. xciii. p. 1010 (1881); *Beiblätter*, vol. vi. p. 39.

then we have, besides the sources of error of the original apparatus, a number of other sources of error which need to be specially examined; so that it is possible the accuracy of the results might be seriously prejudiced. In any case the difficulty of accurately determining the self-induction is the most suspicious part of the method under consideration.

2. The temperature of the coil must be determined with the greatest accuracy, since the conductivity of the wire decreases about 0·3 per cent. for a rise of temperature of 1° C. The corresponding change of length, and consequently of surface embraced, amounts only to $\frac{17}{1000000}$, and may therefore be neglected.

3. We have further to inquire whether there may not be secondary currents induced in the supports of the apparatus, if these are of metal, by the currents circulating in the spiral, which may act upon the magnetic needle*.

This point may be determined by interrupting the continuity of the metallic supports by means of insulating material. According to experiments of this nature, made by Lord Rayleigh and Dr. Schuster †, this source of error was not important in the experiments of the British Association, the error amounting only to 0·16 per cent. It would be better, however, to construct the supports of insulating material.

4. The testing of the instrument to determine if the coils lie symmetrically with reference to the axis of rotation offers no particular difficulty, if we observe by means of a telescope corresponding points of the frame on both sides of the axis in different positions of the coils, making with each other an angle of 180°.

5. In the same way it is easy to ascertain by known optical methods whether the axis of rotation is really vertical, and remains so; deviation from the vertical position may exert a considerable effect.

If the angle of dip were about $\angle = 70°$, an inclination of the axis towards north or south of 0°·2 would cause an alteration in the inductive action of the earth in the proportion of cos 70°: cos (70° ± 0°·2)—that is, not less than 1 per cent. An inclination of this amount in the case of the British-Association coil would correspond to a displacement of 0·5 millim. in the ends of the axis. An exact determination is therefore very necessary. Small displacements of the axis towards east or west have only an insignificant influence.

6. The counting of the number of revolutions of the coil in

* Compare F. Kohlrausch, in Pogg. *Ann.* 1874, Ergänzungsband vi. p. 9.

† *Loc. cit.*

unit time by known methods offers no special difficulty; nor does the maintaining of a constant velocity of rotation by mechanical means*.

7. The adjustment of the magnet in the centre of the rotating coil should also not be difficult to effect. Moreover a small deviation from exact adjustment causes no important error.

8. The moment of the magnet may be determined by vibration-experiments after determining its moment of inertia and the horizonal component of the earth's magnetism; or it may be determined from experiments on deflection. If, in order to render the inductive action of the magnet on the rotating coil imperceptible, we employ magnets of very small moment, then these methods offer many sources of error, on account of the very perceptible influence of the friction of the air in the vibration-experiments, or of the small distance at which the deflecting magnet must be placed.

If the moment of the magnet is very small, it and the distribution of magnetism in the magnet (which is very difficult to determine) have both very small effect upon the results. If the poles of the magnet are at a distance from the central point in the median plane of the coils which is less than $\frac{1}{6}$ of their radius, and if the distance of the poles from the plane is not greater than 0·84 of the length of the magnet, then the force exerted upon it by a current in the coil is constant to within 0·0005, under conditions otherwise similar, up to a deflection of 56°.

9. The adjustment of the telescope and scale required for reading-off the position of the magnet, and the correction of the readings, may be made in the usual way; and the divisions of the scale must be compared with an accurate measure. The accurate measurement of the distance of the scale from the central point about which rotation of the magnet takes place, or from the reflecting surface of the mirror, offers a certain amount of difficulty.

10. The force of torsion of the suspension-thread of the magnet may easily be compared with the directive force of the magnet by turning the thread fastened to a torsion-circle through a certain angle, and observing the deviation of the magnet. Unavoidable changes of considerable magnitude may result from the variable moisture of the air, if weak magnets are employed.

11. We have, further, to inquire what influence currents of air and the vibration of the apparatus caused by the motion

* Compare the ingenious arrangement adopted by Lord Rayleigh, *loc. cit.*

exert upon the needle when the coil is put into rotation with circuit open. In the first experiments of the Committee of the British Association, in which attention had not yet been paid to the separate conditions of the experiment in the way which will be necessary for a final determination of the ohm, these last-mentioned sources of error made themselves in a high degree perceptible.

Thus F. Kohlrausch, in a discussion of these experiments, has justly pointed out that, in order to avoid inductive action on the coil of wire, the magnet (a magnetized steel ball of 8 millim. diameter), in spite of its great mass, had only a moment equal to that possessed by an extremely fine magnetic needle weighing 0·025 gramme. The magnet was attached by means of a wire 0·25 metre long to a mirror of 30 millim. diameter suspended by a simple silk fibre of 2 metres length. The currents of air produced by the rotating coil only 0·31 metre distant acting upon the relatively large surface of the mirror and magnet, as well as the variable torsion of the thread, become much too considerable in comparison with the directive force of the magnet.

Further, the vibration of the apparatus produced by the rotation may propagate itself to the case surrounding the mirror, and so put the air, and with it the mirror, into rotation. It might thus happen that the deflections obtained by rotating the coil in one or the other direction might vary by as much as 8·5 per cent. If the mean results obtained in different series of observations should differ amongst each other by only 2·3 per cent., yet even this is not a guarantee of greater accuracy, but can only be regarded as a proof *that the apparatus always acts in nearly the same way.*

Moreover the supplementary elimination of sources of error, *e. g.* by more exactly calculating the effect of self-induction, as attempted in the memoir of Lord Rayleigh and Dr. Schuster, can in no way free the results from the influence of sources of error shown to exist by the deviations cited above. Above all things, observations of this kind ought never to be arbitrarily corrected on the ground of probability only without having perfectly definite numerical data; or else all secure experimental ground is lost.

Hence we may consider it shown that the results of these experiments are not to be themselves taken as a final determination of the ohm, but rather as extremely valuable preliminary experiments by which we have become acquainted with the precautions to be observed.

By means of new experiments made by Lord Rayleigh and Dr. Schuster with the apparatus of the British Association,

altered in some points, the difficulties mentioned above under (1) remained unaltered; the magnet, however, was replaced by four magnetized needles, each 0·5 centim. long, fastened on the parallel horizontal edges of a cube of cork. The mirror was attached directly to the cork. But here also the directive force of the magnet is small in comparison with the somewhat powerful influence of currents of air acting on the large surface and small moment of inertia.

Further, the case protecting the suspended portions of the apparatus is attached to the glass tube which surrounds the suspension-thread. Perfect stability could hardly be obtained with such an arrangement; since with rapid rotation vibration of the mirror would certainly be experienced. These experiments also are to be regarded more as preparatory for later and final measurements, and as such have been excellently carried out. On this account no doubt the more exact data for vertical adjustment of the axis &c. have not yet been given throughout. Lord Rayleigh, as already mentioned, lays emphasis on the necessity for new coils for the final experiments, and raises the question whether these should not be arranged as in the Helmholtz-Gaugain galvanometer, so that the directive action of the current on the needle is independent of any small excentricity or deviation of the needle.

In the final experiments, whether they are made according to this method or according to one of the other methods, it is in any case necessary that as complete a statement should be given of each separate proceeding in arranging and using the apparatus. Also the experiments should not be made with an apparatus set up and adjusted once for all, since then the errors of the arrangement will repeat themselves with each new determination undertaken with the apparatus. On the contrary, the apparatus itself must be frequently altered in various ways. Only so can we obtain results independent of each other, which can be used for mutual control.

The sources of error which are so difficult to avoid in using method IV., more particularly in consequence of the extra currents, have induced W. Weber * himself, in conjunction with the late F. Zöllner, to again take up the first method (which had been employed by the first-mentioned so long ago as 1846 in preliminary experiments) with the most perfect experimental means. Of two equal coils, weighing some 207 kilog., most carefully and very regularly wound upon mahogany rings, as shown by measurement of different diameters, the

* W. Weber and F. Zöllner, *loc. cit.*

coils consisting of cotton-covered copper wire about $3\frac{1}{3}$ millim. in thickness, and having a resistance of about 5 ohms—one, the multiplier, surrounds a cylindrical magnetic needle about 100 millim. long and 10 millim. thick, fixed in the magnetic east and west plane. The diameter of the coils (interior about 960 millim., exterior 1040 millim., and breadth of coils about 254 millim.) is so large that the magnetic forces acting on the needle may be supposed, without perceptible error, to be concentrated at their centre. The other coil, connected with the first by conducting-wires, the inductor, is capable of rotation about a vertical axis, out of the east and west position through an angle of 180°. This displacement of the inductor takes place suddenly at such times that the currents induced in it and transmitted through the multiplier render the deflections of the needle constant according to the "method of multiplication" or the "method of recoil."

In carrying out the experiments, the displacement of the inductor by means of clockwork was not found to be practicable, and it was therefore effected by hand. The time required for the displacement (some 2 seconds) must only be a small fraction of the time of vibration of the magnet hanging in the multiplier. For this purpose the time of vibration of the needle is increased (up to 30 seconds) by placing the needle in a stirrup to which a tube of brass 272 millim. long was fastened in a horizontal position and at right angles to the needle, the ends of which carried plane mirrors. Telescopes with parallel scales are placed before both mirrors, each at a distance of 4 millim. By this double reading the difficult measurement of the scales from the mirrors is replaced by the easier measurement of the distance of the scales from each other, and of the distance of the mirrors apart. An examination of the point whether, and how far, the instants of displacement of the inducing coil may differ from those indicated by calculation, and how far these possible deviations may affect, might be easily made*. In any case, according to the experiments which have been made, this influence cannot be of importance, since the results obtained at different times by use of a magnet 100 millim. long agree with each other to within 0·06 per cent., and those obtained with a magnet 200 millim. long do not differ at all from the mean result. This shows at the same time that the instrument had not altered in any important respect in the interval. The inductor must be so far removed from the multiplier that the currents in the first cannot directly deflect the needle.

* Compare Chwolson, *Bulletin de St. Pétersbourg*, vol. ii. p. 403; *Beiblätter*, vol. v. p. 450.

It is a point of very great importance in the employment of this method that self-induction in the inductor is without influence, and that, further, the intensity of the earth's magnetism does not enter into the calculation, provided that it is of equal intensity in the positions occupied by inductor and multiplier. It may not be allowable to assume that this is the case in ordinary rooms, because of iron hooks let into the walls; but in a building constructed for the purpose this condition might easily be fulfilled. This point may also easily be controlled by oscillation-experiments; the ratio of the directive force of the earth may be determined at the two places.

It is true that we have to set against this advantage that we have two coils to measure, and are so liable to a double error. (This, however, is also the case in the experiments of the British Association described above, where the revolving coil consists of two separated halves.) This disadvantage may be considered small in comparison with the error resulting in the fourth method from the measurement of the self-induction of the coil. It is further reduced by the large dimensions of the coils. At the same time stability of the apparatus, and consequent freedom from disturbances resulting from vibration and from currents of air, is much more easily obtained than with the rapidly-rotating coil employed in the former method.

Previous communications on experiments with this apparatus are only preliminary, on which account we are not yet in possession of details of adjustment; for which, however, not only the name of W. Weber, but also that of Repsold, the instrument-maker intrusted with the work, give us full guarantee that such points as the verticality of the axis of rotation, the adjustment of the coil magnetic east and west, the arrangement for displacing the coil through exactly 180°, and so on, will be attended to. The preliminary method of measuring the circumference of the mahogany rings on which the coils are wound and of their exterior circumference by means of strips of paper, which were then compared with a divided wooden rule, will no doubt give place to more exact methods in the actual experiments.

A special advantage of this method is that the wires may be unwound from the rings and wound again without any great difficulty; and so repeated observations independent of each other can be obtained.

This method is therefore to be recommended for the final determinations.

If we employ Weber's second method, we have no need to determine the dimensions of the multiplier; but we determine

the damping of the oscillations of the needle, once with open circuit and again with closed circuit. Hence, by determining the deviation of the needle when the inductor is displaced, the resistance of the circuit in electromagnetic measure may be calculated. For this purpose the multiplier must enclose the needle more closely, so that the damping may be sufficient. We have to determine, besides the dimensions of the inductor, the period of oscillation and moment of inertia of the needle and the intensity of the horizontal component of the earth's magnetism. The sources of error of this method have been carefully examined by F. Kohlrausch *; the error in measurement of the inductor may, however, have been scarcely estimated high enough. (Compare also the following method.)

If proper experimental means and a suitable observatory are available† for accurate determination of the earth's magnetism, this method may very well be employed together with Weber's first method.

In Weber's third method, which is apparently so simple, we have, in order to determine the absolute resistance of a multiplier, to observe only the damping of the oscillations of a needle suspended in the coil when the ends of the coil are united, and again when the circuit is open. In order to be able to calculate the electromotive force due to the oscillations of the needle and its action upon the needle, which is directly proportional to that electromotive force, and inversely proportional to the resistance to be measured, we must know in general the dimensions of the multiplier and the position of the needle relative to it, as well as the distribution of the magnetism in the needle.

The former determinations are difficult to carry out exactly, since, in order that there may be sufficient damping, no very large dimensions can be given to the coil; the latter determinations can only be made inexactly by observation of the currents induced in a short coil at different points of the magnet, or by numerous oscillation- and deflection-experiments with a magnetic needle swinging at different distances from various points of the magnet. We have also to satisfy ourselves that the damping is not dependent upon the angle of deflection of the needle.

This method has been employed by Fr. Weber ‡, in Zürich,

* *Loc. cit.*

† Compare also the new methods by F. Kohlrausch, *Göttinger Nachrichten*, March 4, 1882.

‡ F. Weber, *Elektromagnetische und kalorimetrische Messungen* (Zürcher und Furrer, Zürich, 1878); *Beiblätter*, vol. ii. p. 499.

employing a magnet of 80 millim. length, 20·1 millim. breadth, and 21·1 millim. depth, suspended by a silk thread between two conical coils very regularly wound, of 144·43 millim. internal, 184·46 millim. external radius, and 51·64 millim. breadth.

The mean distance of the spirals from each other was either very small, or the distance of their median planes was made 164·4 millim., or very nearly equal to the mean radius of the coils. In this last case the distance of the poles from each other has no important influence, although it has in the first, where also the distribution of moments in the magnet must not be neglected. We must, however, always take into account inevitable differences in measurement in consequence of the mutual pressure of the coils and certain unavoidable irregularity in the winding, which are more important in consequence of the small dimensions of the coils, as in the fourth method which we have already considered. Nevertheless it was shown, by comparison with a standard resistance of Siemens's, with which the resistance of the coils was compared by means of a Wheatstone's bridge, in both the cases mentioned, and after rewinding of the coils, that the standard resistance expressed in absolute measure varied in three series of experiments only from 0·9532 to 0·9570, from 0·9528 to 0·9555, and from 0·9527 to 0·9551—that is, not more than 0·5 per cent. The mean is 0·95451.

In further experiments according to another method, Fr. Weber placed the coils mentioned above at a definite distance from each other, connected the one, "the inducing coil," with a very constant Daniell's cell and a simple ring of 168·7 millim. radius; the other, "the induced coil," with a multiplier, which was composed of a coil consisting of two equal conical halves placed close together, of 154·2 millim. inner radius and 172·2 external radius, between which was suspended a magnet 40 millim. long provided with a mirror. The simple ring of the inducing circuit lay between these coils. The deviation of the needle whilst a constant current passed through the inducing circle measured its intensity J, which was to be measured in absolute measure from the known dimensions of the ring; the deviation on breaking the inducing circuit after disconnecting the ring gave the intensity i of the induced current. Since the induced electromotive force $e=JP$ (where P is the mutual potential of the coils) may be calculated in absolute measure from the dimensions of the coil by putting the constant of induction equal to unity, we obtain the resistance of the induced circuit r from the formula $r=\frac{e}{i}$.

We have therefore for these determinations to ascertain the dimensions (1) of the wire ring, (2) of the inducing coil, (3) of the induced coil, (4) of the multiplier, (5) of the distance of the separate layers of the inducing and induced coils from each other, (6) the determination of the position of the magnetic poles, to which are to be added the manipulations required for the other methods, the adjustment of the needle in the centre, and of the coils of the multiplier and of the wire ring between them with reference to the meridian &c. Since the intensities of the primary and secondary currents are measured by the deflection of the same magnet, the horizontal component of the earth's magnetism does not enter into the calculation. There are in this method many more sources of error to be taken into account than in the other methods, by which its accuracy may be seriously impaired.

Notwithstanding, Weber finds, by determining the resistance of the inductive circuit by this method, and comparing it with the Siemens standard which he employed, that at two different distances of the inducing and induced coils, and with two different intensities of the inducing current, a mean result of 0·9554 (0·9589 to 0·9516)*.

This method has also been employed by Rowland†, only that he employed a tangent-galvanometer to determine the intensity of the inducing current, but a special galvanometer to determine the induced current; it was necessary also to determine the ratio of the intensities of horizontal force at the position of the two pieces of apparatus. His three induction-coils had a diameter of about 27·4 centim., and were wound on frames having a thick brass rim on each side. They could be combined in pairs, so that the one which served as inductor, through which a constant current flowed which could be interrupted, might be placed at four different distances from the others (6·5 up to 11·47 millim.). Very thin-covered copper wire (no. 22) was taken for the coils; so that the sources of error mentioned on p. 260, caused by displacement of the covering, the mutual compression of the coils, and expansion of the wire, become very prominent. Since the in-

* An indirect method employed by Fr. Weber to determine the resistance of a zigzag platinum wire placed in a calorimeter, from the heat produced by a current whose strength in absolute measure was determined by means of a tangent-galvanometer of known dimensions, can hardly be placed by the side of the more direct methods, on account of the difficulty of all calorimetric measurements. Nevertheless Fr. Weber finds by this method, by comparison with his Siemens standard, a resistance for the latter of 0·9560, which again differs only 0·1 per cent. from the other determinations.

† Rowland, 'Silliman's Journal,' 1878 (3), pp. 325, 430.

duction-coils are so close together, a small error in measuring their distance apart is of great influence—the more so since, from what has been said, it follows that the position of the separate coils could not be accurately controlled. That the distance of the coils apart at various points was measured to the $\frac{1}{20}$ millim., and that the mean distance apart is given to $\frac{1}{1000}$ millim., and the mean radius to the $\frac{1}{10000}$ millim., can hardly be regarded, in view of the above-mentioned disturbing causes, as a guarantee of the actual exactitude of the results.

Since the length of the needle of the tangent-galvanometer (2·7 centim.) amounted to only $\frac{1}{18}$ of the diameter of the coil (50 centim.), the deviation from the law of tangents is imperceptible ; but, on the other hand, there is an important source of error in the fact that the needle turned upon a point. Rowland asserts that the needle, which was provided with a pointer playing over a circle of 20 centim. diameter, always took up its position accurately to 1 or 2 minutes (1 minute corresponds to only 0·03 millim. upon the circle). Since this does not at all agree with previous experience, an exact explanation how so great sensitiveness was obtained is indispensably necessary.

The needle of the galvanometer was 1·25 centim. long ; the coils, which were brought up to it from the east and west, had an internal diameter of 3 centim. and an outer diameter of 5·6 centim. ; their inner end-surfaces were at a distance of 0·935565 centim. (thus given to $\frac{1}{100000}$ millim.) from the point on which the needle turned. Whether the law of tangents is altogether applicable here must remain uncertain.

Since the intensity of the inducing and induced currents is measured at different places, Rowland introduces a further complication by determining the ratio of the horizontal component of the earth's magnetism at the position of the tangent-circle and of the galvanometer by surrounding the galvanometer with a larger coil, sending the same current through it and the tangent-circle, and comparing the deflections. The radius of the new circle (some 4 centim.) is again given to $\frac{1}{10000}$ millim.

The values of the different dimensions given to so many decimal places are probably not regarded by the author himself as accurate in the same degree, the last decimals (obtained by interpolation) being added for the sake of completeness. It is, however, always advisable to give exact account how far actual observation goes, since the mean values can never be accurate beyond the limit of possible observation, or else with the roughest measures it would be possible by repeating measurements to obtain any required degree of minuteness.

Since, moreover, the sources of error mentioned above may undoubtedly cause considerable deviations, such a one-sided accuracy cannot be a guarantee for the exactitude of the final results *.

On account of the much larger number of determinations which this method requires, I consider it much less suitable for the final construction of a definite resistance than the fourth and first of Weber's methods, of which again I give the preference to the latter.

After a conductor A of known absolute resistance has been constructed, by the one method or the other, it has to be compared with the resistance B of a mercury column of known dimensions in order to determine the length of such a column of 1 square millimetre section which possesses 1 ohm resistance.

If we employ Weber's first method, we may introduce the mercury column B directly between the inductor and multiplier, and calculate the resistance of B from the decrease of intensity; otherwise the resistances A and B may be compared by means of the differential galvanometer, or more conveniently by means of the Wheatstone's bridge, according to the accurate method already employed by W. Siemens in constructing copies of his mercury unit. Since we already know the length of mercury corresponding to 1 ohm very nearly, we take care to arrange the resistance B so as to be as nearly as possible equal to the resistance A.

It is highly important, after calibration of the measuring-wire, to secure perfect contacts by using freshly-amalgamated thick copper connecting-pieces and cups full of pure mercury, the resistance of which is previously determined by including several in a circuit. Plug-contacts are not sufficiently accurate. Also the temperature must be carefully kept constant throughout at the right point, as was the case in the construction of Siemens's copies.

* If we employ in this and other methods a coil wound uniformly on a closed ring, a neutral solenoid, as inductor, but as induced coil a coil surrounding the former at one point, the conditions are so far simpler that the current due to opening or closing the current in the solenoid, only the linear dimensions of the solenoid, its interior and exterior diameters d and d_1, the number of coils of the induction-coil and of the solenoid, or in the case of a solenoid of rectangular section the height a, are concerned (compare Roiti, *Atti di Torino*, vol. xvii., 30th April 1882). Nevertheless the employment of such a solenoid is attended with the great difficulty of winding the wire exactly upon it, and of measuring its dimensions with sufficient accuracy. The latter is the more difficult, since the section of the solenoid can only be taken relatively small, and the formula contains the expressions $\sqrt{d}-\sqrt{d_1}$ for a solenoid with circular section, $a\log\frac{d_1}{d}$ for a solenoid with rectangular section.

A difficulty presents itself here, since induced currents are produced in A on opening and closing the current which flows through the combination of wires. We must therefore either employ a constant current of such feeble intensity that the heating of the circuit shall be imperceptible, or, according to the method of Kohlrausch*, who has also worked out the necessary calculations, we must employ an induction-apparatus which sends alternating currents through the circuit. A special source of error is found in making contact with the mercury column contained in an accurately-calibrated carefully cleaned glass tube. The ends of the tube terminate in glass vessels of diameter relatively large. The apparatus is best filled with mercury by pouring mercury into one of the vessels and inclining the apparatus towards the same side, then, after closing the openings, repeatedly exhausting the apparatus as completely as possible, gently warming the tube, then allowing pure air which has passed over phosphoric anhydride and wadding to enter, and finally, after exhausting again, allowing the mercury to flow into the tube by placing it in a horizontal position. If the electrodes are immersed in the vessels containing mercury, then the resistance of the mercury in the tube is increased by that of the mercury contained in the vessels up to the ends of the electrodes. This resistance may be calculated on the assumption that the vessels are infinitely large. It might perhaps be advantageous to determine it directly by compensating the resistance of a tube filled with mercury with two contact-vessels by a nearly equal resistance on Wheatstone's bridge, and then to cut the tube at one or more points and to introduce wider mercury vessels of suitable form, and then again determine the resistance. We might also introduce two tubes, of lengths m and n and of equal section, between the same contact-vessels, and compare their resistances, and from the data so obtained calculate the resistance of the vessels.

In order to avoid this separate comparison of the resistance of the coil determined in absolute measure with a mercury column, Carey Foster † and Lippmann ‡ have almost simultaneously suggested (L.) or carried out (C. F.) the ingenious plan of employing Weber's fourth method, as adopted by the British Association, and Poggendorff's compensation-method

* Poggendorff's *Annalen*, 1871, vol. cxlii. p. 418.

† Carey Foster, 'Electrician,' 1881, vol. vii. p. 266; *Beiblätter*, vol. vi. p. 133.

‡ Lippmann, *Comptes Rendus*, 1881, vol. xciii. p. 713; *Beiblätter*, vol. vi. p. 43.

for determining electromotive force without having previously determined the resistance of the rotating coil.

Let the resistance R to be investigated and a tangent compass be included in the circuit of a constant pile, *e. g.* a thermopile. Let the ends of the resistance be connected also by a second circuit, into which a delicate galvanometer and the coil rotating about a vertical axis are introduced during a particular phase of its motion by means of a commutator revolving with the coil. The velocity of rotation is so regulated, or the resistance in the branch containing the constant pile is so altered, while the velocity of rotation of the coil remains constant, that the galvanometer is not affected. Then, at the moment that the coil is introduced, the induced electromotive force in it is equal to the difference of potential at the ends of the conductor R caused by the thermopile. If the current-intensity in it, and also in the circuit of the thermopile and of the tangent-galvanometer, be equal to J, then $E=JR$. The factor of reduction of the tangent-galvanometer is to be determined from its dimensions and the horizontal component of the earth's magnetism at the place, and in the same way the electromotive force induced by the rotation of the inductor. In most cases we may assume the horizontal component of the earth's magnetism to be the same at both places, as in Weber's first method, which thus disappears from the calculation. In this method, therefore, we have the same determinations to make as in Weber's fourth method. The uncertainty of contact in the commutator is without influence, since when the galvanometer is not affected there is no current in the inductor-circuit. If we adjust the commutator so that it makes contact with the coil at the time of maximum induction, just at the moment when it varies least, then, if the ratios are in other respects properly chosen, the influence of self-induction in the rotating coil is reduced to a minimum. A certain amount of care, however, is required in order to avoid thermo-electric disturbances, resulting from the heating of the points of contact; and the determination of the exact phase during which the commutator makes contact offers a certain amount of difficulty.

The results so far obtained by the different methods are, some of them, tolerably far apart, although the investigations have been carried out with great care, if not in all points with the most perfect experimental means which a final determination of the ohm demands. Thus the resistance of a mercury column 1 metre long of 1 square millim. section at 0° C. was found by the first experiments of the British Association to

be 0·9830; according to F. Kohlrausch it is 0·9717; according to F. Weber 0·9550; and according to Lord Rayleigh and Mrs. Sidgwick 0·9413 $\left(10^9 \frac{\text{centim.}}{\text{sec.}}\right)$. The first ohm constructed by the British Association, according to F. Kohlrausch is 1·0196; according to Rowland 0·9910, and according to Rayleigh and Schuster 0·9893 of the true ohm $\left(10^9 \frac{\text{centim.}}{\text{sec.}}\right)$.

Hence at any rate it is indicated that the final determination of the ohm must not rest alone on experiments made only according to one method and carried out at one place. Further, the results of each separate method (as I have already mentioned) offer security against possible constant errors only if they are obtained from entirely independent series of experiments, made with apparatus varied in all possible ways. Since investigations are already in progress in different places, with excellent apparatus and according to different methods, we may shortly expect to be in a position to compare together the data which they yield, and so to attain as reliable a final result as possible.

In such important and permanent determinations as those of the electrical units, a delay of a few months is of no importance whatever in comparison with the reliability of the result to be obtained. Any introduction of the ohm as given by detached series of observations and distribution of copies for practical use would therefore be premature and without authority.

The Commission appointed for the determination of the electrical units does not complete its task by simply determining the ohm. It is further necessary to construct at least one of the two remaining units, *e. g.* the volt. Since it is at present impossible to construct this in a form capable of reproduction, it will be necessary first to compare electromotive forces with those of a constant element whose electromotive force is known in volts. We are therefore in the same case as if, wishing to measure a length in metres, we were obliged to employ a yard measure whose ratio to the metre was known. So much the more thoroughly are we obliged to investigate the electromotive forces of the galvanic cells to be employed as intermediate measures, and their dependence upon external conditions. The cell invented by Latimer Clark has indeed been shown to be capable of reconstruction of exactly the same electromotive force; at the same time it can only be employed for electrostatic measurements, since it becomes polarized when used to produce a current. Obser-

vations with reference to Daniell's and other cells do not appear to me to lead to any certain conclusion.

Similar indirect methods have to be employed for measuring the intensity of a current in absolute measure; the factor of reduction of the tangent-galvanometer employed, for example, has to be determined. This problem has been considerably facilitated by the recent investigations of F. Kohlrausch and Mascart on the electrochemical equivalent of silver, which, however, exhibit small deviations among themselves. There is here consequently a rich field for accurate investigation, for which we have already a series of valuable preliminary investigations. All these labours, however, can only give a final result sufficient for our present purpose when they are executed upon some common and well organized plan, and are carried out with the most perfect experimental means.

XXXI. *Crystallographic Notes.* *By* L. Fletcher, *M.A., of the Mineral Department, British Museum; late Fellow of University College, Oxford**.

[Plate VI.]

X. *On Twins of Copper Pyrites.*

THE memoir of Haidinger† on the Crystallisations of Copper Pyrites, published so long ago as the year 1822, was so exhaustive and withal so simple in its character that little seemed to be left to tempt the crystallographer to devote further study to this mineral; and in fact, with the exception of the papers of Sadebeck, whose early death all interested in the progress of mineralogy must so deeply deplore, and the confirmatory data in Groth's Catalogue of the Collection of the University of Strassburg, we have still no other information at our disposal. A study of these memoirs and of the various text-books of mineralogy leaves upon one's mind such a feeling of doubt as to the true statement of one of the laws of twin-growth, and that (as will be explained later) a law almost, if not quite, unique in the domain of crystallography, that, at the suggestion of Professor Maskelyne, the collection of copper pyrites in this Museum has been examined with a view to a possible settlement of the difficulty.

To get a clear idea of the present position, it is necessary to trace the history of this particular law from its first statement down to the present time.

In the above memoir of 1822, the twin-growths of copper

* Read before the Crystallological Society, June 3, 1882.

† Memoirs of the Wernerian Society, vol. iv. p. 1, 1822.

pyrites were assigned by Haidinger to three distinct laws. In the first kind, the twin-plane, or plane of rotation, is a face of the octahedron $\{1\,1\,1\}$, and the composition-plane, or plane of junction, is generally parallel, but sometimes perpendicular, to the twin-plane: these growths correspond to the blende twins of the Cubic system. In the second, the twin-plane is a face of the octahedron $\{1\,0\,1\}$, and the composition-plane is *perpendicular* to the twin-plane. In the third, the twin-plane is a face of the prism $\{1\,1\,0\}$, and the individuals are interpenetrant. The second law is that with respect to which uncertainty has arisen; and it is with this law that we have now to deal.

In the original memoir of 1822 the growth is so clearly described, and the law so distinctly expressed, that it is impossible to read the memoir and to mistake its meaning. Three years later, however, Haidinger published in the 'Edinburgh Journal of Science' a series of papers "On the Regular Composition of Crystallised Bodies," copiously illustrated with figures which have since found their way into almost every manual of the science; and in its natural place in the tetragonal (or pyramidal) system he describes once more, though briefly, the particular growth of copper pyrites which we are about to consider. There is little doubt that this secondary description has been the cause of serious misunderstanding.

On page 68 of vol. iii. (1825) we read as follows:—"Regular composition often also takes place in this species parallel to a plane of $P-1\{1\,0\,1\}$, or perpendicular to the terminal edges of $P\,\{1\,1\,1\}$. There are particularly two varieties of *this case* which in the present place deserve our attention. The individuals are either joined in pairs, or one central individual is surrounded by four others, added in the direction of all the edges of P. The product of the first, in the fundamental pyramid, would be fig. 30 [similar to fig. 6]. This has not yet been observed; but it will serve for explaining fig. 31, a variety of the form $P-\infty\,\{0\,0\,1\}$, $P-2\,\{1\,1\,2\}$, $P\,\{1\,1\,1\}$, $\frac{3}{2\sqrt{2}}P\,\{3\,0\,2\}$, and $\frac{3}{2\sqrt{2}}P+1\{3\,3\,2\}$; from the mines of the district of Siegen in Prussia. This and several other interesting varieties of forms from the same locality I have described on another occasion (*Mem. Wern. Soc.* vol. iv. part 1, p. 1, 1822), from specimens in the possession of Mr. Sack, of Bonn."

Taken by itself, this explanation might at first sight suggest the interpretation which seems to have been placed upon it by some crystallographers—namely, that there are twin-growths of copper pyrites in which the *plane of composition* is parallel to a plane of the octahedron $\{1\,0\,1\}$.

This interpretation can never have been intended by Haidinger. At the beginning of the series he had remarked that, for the precise definition of a twin-growth, two planes must be given:—first, the twin-plane, or plane of rotation, to indicate the relative *directions* of corresponding faces of the two individuals; and, secondly, the composition-plane, or plane of junction, to indicate their relative *positions*. Haidinger's initial sentence, "regular composition takes place parallel to a plane of {1 0 1}, or perpendicular to the terminal edges of {1 1 1}," may therefore refer either to the plane of twinning or to that of composition, and to this extent is indefinite. But seeing that no plane of {1 0 1} is perpendicular to a terminal edge of {1 1 1}, the above sentence must indicate two distinct cases if the reference be to a composition-plane, while if the reference be to a twin-plane, only a single case is indicated; for, as will be shown later, a rotation through two right angles in a plane of the octahedron {1 0 1} is crystallographically identical in its results with a rotation through two right angles perpendicular to (or round) a terminal edge of the octahedron {1 1 1}. That Haidinger only recognizes a single case is evident from the italics in the next line of the above quotation. As a matter of fact, the figure of the twinned octahedron, repeated from his first paper, shows the plane of composition as perpendicular to the twin plane, though this is only evident after careful inspection. His final reference to the original memoir without calling attention to any deviation therefrom, shows that he was still in accord with the explanation there given.

From this we conclude that either a careful study of this later paper of Haidinger on the regular composition of crystallised bodies, or a simple reference to his original memoir on the crystallisations of copper pyrites, would have made clear the fact that Haidinger regarded the composition-plane as perpendicular to the twin-plane (1 0 1), though we grant that at first glance his second paper might suggest that the composition-plane is, in some cases at least, parallel to the plane of twinning (1 0 1).

In 1830, Naumann's well-known work* did in fact state the law differently from Haidinger, and assumed the plane of composition to be *parallel* to that of rotation. In his preface Naumann makes special mention of the help he had derived from Haidinger's series of papers on regular composition, and in his description of these particular growths adopts some of the figures there given. It seems quite impossible for this deviation from Haidinger to have been introduced wittingly;

* *Lehrbuch der reinen und angewandten Krystallographie*, 1830.

for attention is not directed to the difference in the explanations, and, further, it is unlikely that Naumann had been able, so soon after the publication of Haidinger's work, to undertake a special and minute study of this particular law.

We feel that the above reasoning shows clearly enough that Naumann's statement is a simple misinterpretation of that of Haidinger, and that this can only have resulted from the fact that Naumann referred to the secondary, and not to the original, explanation given by the author of the law.

As Naumann's 'Crystallography' became the recognized and universal text-book on the subject, this statement, though a mistaken one, has been extensively circulated, and appears probably in every text-book of the present day, though, as might have been expected, the explanation of 1822 is repeated in Haidinger's own manual of 1845.

In 1868* Sadebeck published the results of his study of specimens of copper pyrites (belonging chiefly to the Berlin collection), and in the explanation of the twins assumed the correctness of Naumann's statement of the law. In a second paper, published in the following year†, he gives an explanation of his position, so very brief and so clearly illustrative of the present difficulties that a translation is given here:—

"In my memoir on copper pyrites I have wrongly stated the law of twinning; for I have supposed the twin-plane to be also the composition-plane. According to this, one pair of tetrahedron-faces should meet in a salient angle of 1° 24′, and the opposite pair in a reentrant angle of the same magnitude. After I had published the memoir, Haidinger informed me by letter that this was not the explanation he himself had given, as may be seen from his statement in the 'Edinburgh Journal of Science,' which runs thus:—'Composition takes place perpendicular to the terminal edges of P.' In consequence of this friendly private communication from so famed a Nestor of the science, I subjected the crystals again to a careful study. The result was that I found it impossible to say whether the tetrahedron-faces actually coincided, or formed an angle of 1° 24′. This led me to retain my old view, since that law seemed to me a simpler one which regarded a plane of the form $\{1\,0\,1\}$ as at once twin-plane and composition-plane. But if I now apply the general law for tetrahedral twins to this case, it follows that, as faces of the positive tetrahedron of the one individual are adjacent to faces of the

* "Ueber die Krystallformen des Kupferkieses," *Zeit. d. deutsch. geolog. Gesellsch.* p. 605, vol. xx. 1868.

† "Allgemeines Gesetz für tetraëdrische Zwillingsbildung," *Zeit. d. deutsch. geolog. Gesellsch.* p. 642, vol. xxi. 1869.

positive tetrahedron of the other, the composition-plane is perpendicular to the twin-plane. The tetrahedron-faces therefore must really fall into a plane; and I hope that crystals may yet be found which will leave the matter beyond doubt."

From this we conclude:—first, that Sadebeck had followed Naumann, assuming his explanation to be that of Haidinger; secondly, that, owing to the practical difficulty of distinguishing between growths according to the two laws, he could come to no decision from simple examination of the specimens; and, thirdly, that he declared in favour of Haidinger's view merely that the law might not be at variance with a second law, which was true in certain cases, but of which the general application had not been proved.

Apparently before 1876 there was another change of view on the part of Sadebeck; for on page 82 of Rose and Sadebeck's 'Crystallography'*, where this law is briefly referred to, we read that "the individuals have a face of the form $\{1\,0\,1\}$ for composition-plane;" and a footnote gives a reference to the first paper of Sadebeck, without stating whether or not he had since obtained that evidence of the incorrectness of Haidinger's explanation which was confessedly wanting so late as the time of publication of the second. The omission of any reference to the difficulty may have arisen from unwillingness to perplex the student of an elementary text-book.

The next mention of the law is made in Groth's Catalogue of the Strassburg Collection (1878). We there find that, "as for the regular growths of copper pyrites, the results of Sadebeck are quite confirmed by the specimens in the Strassburg collection;" and further on we read that these particular growths are symmetric twins about a plane of the form $\{1\,0\,1\}$. In other words, Sadebeck's *first* explanation, or that of Naumann, is accepted. There is no reference to the difficulty in which Sadebeck had found himself placed; indeed it is quite possible that the later explanation of Sadebeck, agreeing with that of Haidinger, had escaped notice owing to its having appeared in a paper dealing with a more general subject and having no reference to copper pyrites in its title: in any case no measurements are recorded which render it possible to distinguish between the two statements of the law.

In the latter part of the same year, according to a paper on haplohedral hemihedry †, either Sadebeck was in a state of doubt as to which is the correct explanation, or else he considered both correct; for we read as follows:—"If, on the

* Rose and Sadebeck's *Elemente der Krystallographie*, 1876.

† "Ueber geneigtflächige Hemiëdrie," *Zeit. d. deutsch. geolog. Gesellsch.* p. 601, vol. xxx. 1878.

one hand, a face of {1 0 1} be both twin-plane and composition-plane, the adjacent tetrahedron-faces form small salient or re-entrant angles; if, on the other hand, the composition-plane be perpendicular to the twin-plane, the tetrahedron-faces of the one are coincident with the adjacent faces of the other." The probability is that both are mentioned, not because Sadebeck believed them to be both true, but merely to show that in either case the position then being contended for was a tenable one; and in fact the position of the composition-plane has no further bearing on the argument of that paper.

It would at first sight appear that a difference of a right angle in the position of the plane of composition would manifest itself by angular differences in the twin sufficient to render any difficulty of distinction impossible; we shall therefore attempt to make quite clear what differences would be observed in growths characterized by such different laws.

Fig. 2 represents an octahedron {1 1 1} of copper pyrites in equipoise, $a\ b\ c\ d$ being one set of similar alternate faces, and $\alpha\ \beta\ \gamma\ \delta$ the other set respectively parallel to the first. This figure approaches very nearly to the regular octahedron of geometry and of the Cubic system, of which the faces are all equilateral triangles and the sections through the edges all squares: the difference therefrom was first made known by Haidinger in the memoir of 1822. Though $A\ B\ \bar{A}\ \bar{B}$, the basal section of an octahedron of copper pyrites, is a square, the sections $C\ A\ \bar{C}\ \bar{A}$, $C\ B\ \bar{C}\ \bar{B}$ through the terminal edges are merely rhombs, the angles in the vertical axis being $90°\ 51'$, and in the horizontal axes $89°\ 9'$; the triangular faces are only isosceles, the vertical angle of each being $60°\ 29'$ and the basal angles $59°\ 45\frac{1}{2}'$.

In fig. 1, $a\,b\,c\,d$ and $\alpha\,\beta\,\gamma\,\delta$ are the stereographic projections of the points in which lines drawn through the centre parallel to the normals of the faces of the octahedron of fig. 2 would meet the sphere.

The faces of the form {1 0 1} truncate the terminal edges of the octahedron {1 1 1}; $T\ \bar{T}\ Q\ \bar{Q}$, four of the poles of this form, are shown in fig. 1.

According to Haidinger's measurement, $2\,Ca$ is $108°\ 40'$, whence

$$\tan QC = \tan Ca \cos Q\,Ca = \tan Ca \cos 45°.$$

Thus

$$QC = 44°\ 34\tfrac{1}{2}',\ QT = 90°\ 51',\ \text{and } Q\,a = 35°\ 3\tfrac{3}{4}';$$

also

$$\cos Ta = \cos Q\,a \cos Q\,T,$$

and

$$Ta = T\beta = 90°\ 41\tfrac{3}{4}',$$

while

$$T\alpha = Tb = 180° - 90°\ 41\tfrac{3}{4}' = 89°\ 18\tfrac{1}{4}'.$$

Fig. 3 represents a second crystal, with its faces $a_1 b_1 c_1 d_1$ $\alpha_1 \beta_1 \gamma_1 \delta_1$ parallel respectively to the faces $a\, b\, c\, d\, \alpha\, \beta\, \gamma\, \delta$ of fig. 2.

Both versions of the law assume that the plane of rotation or the twin-plane is a face of the form $\{1\,0\,1\}$. Let us take for the particular plane of rotation the plane $(1\,0\,\bar{1})$ which truncates the edge $\delta_1 c_1$, or the edge $\delta\, c$, and is represented in the stereographic projection by its pole T.

On rotating the crystal represented in fig. 3 through two right angles round the normal $\mathrm{T}\,\bar{\mathrm{T}}$ to the plane $(1\,0\,\bar{1})$, its faces will take up positions represented in fig. 4; and the poles of the faces in this new arrangement are introduced in fig. 1 as $a_1 b_1 c_1 d_1 \alpha_1 \beta_1 \gamma_1 \delta_1$. In the first place we may remark that as each of the pairs of faces $\delta_1 c_1$ and $\gamma_1 d_1$ is diagonally symmetrical to the line $\mathrm{T}\,\bar{\mathrm{T}}$ about which the rotation takes place, c_1 will after the rotation have a direction parallel to that belonging previously to the face δ_1, and still belonging to the face δ; and similarly the face d_1 will after the rotation have a direction parallel to that belonging previously to the face γ_1 and still belonging to γ. In other words, the faces $c_1 d_1 \gamma_1 \delta_1$ of the rotated octahedron shown in fig. 4 are respectively parallel to the faces $\delta\, \gamma\, d\, c$ of the octahedron of fig. 2. If the octahedron had been the regular one of geometry, not only these faces, but all the remaining faces and all the edges of the octahedron of fig. 4 would have been parallel to faces and edges of the octahedron shown in fig. 2. As the line $\mathrm{T}\,\bar{\mathrm{T}}$ bisecting $\bar{\mathrm{C}}\,\mathrm{C}$ is perpendicular to the edge $\bar{\mathrm{C}}\,\mathrm{A}$, and therefore not parallel to the edge C A, the point T will not be midway between A and $\bar{\mathrm{C}}$; and thus, although the edge $\mathrm{A}\,\bar{\mathrm{C}}$ would be unchanged in direction by a rotation through two right angles about $\mathrm{T}\,\bar{\mathrm{T}}$, while B would be rotated to $\bar{\mathrm{B}}$ and $\bar{\mathrm{B}}$ to B, yet $\bar{\mathrm{C}}$ would not be rotated exactly to A nor A exactly to $\bar{\mathrm{C}}$; and the edge C A of fig. 2 will thus not be parallel to $\bar{\mathrm{A}}_1 \bar{\mathrm{C}}_1$ of fig. 4, nor the edge B A to the edge $\bar{\mathrm{B}}_1 \bar{\mathrm{C}}_1$. In fact, while the edge $\mathrm{A}_1 \bar{\mathrm{C}}_1$ is parallel to the edge $\bar{\mathrm{C}}\,\mathrm{A}$, the angle $\mathrm{A}_1 \bar{\mathrm{C}}_1 \bar{\mathrm{A}}_1$ is, as stated above, 90° 51′, and the angle $\bar{\mathrm{C}}\,\mathrm{A}\,\mathrm{C}$ is 89° 9′; whence the edges $\bar{\mathrm{C}}_1 \bar{\mathrm{A}}_1$ C A must have a mutual inclination of 1° 42′. Similarly, although the edge $\mathrm{A}_1 \bar{\mathrm{C}}_1$ is parallel to the edge $\bar{\mathrm{C}}\,\mathrm{A}$, and the plane $\bar{\mathrm{C}}\,\mathrm{A}\,\mathrm{B}$ to the plane $\mathrm{A}_1 \bar{\mathrm{C}}_1 \bar{\mathrm{B}}_1$, the angle $\mathrm{A}_1 \bar{\mathrm{C}}_1 \bar{\mathrm{B}}_1$, as stated above, is 60° 29′, and the angle $\bar{\mathrm{C}}\,\mathrm{A}\,\mathrm{B}$ is 59° 45½′, whence the edges B A $\bar{\mathrm{B}}_1 \bar{\mathrm{C}}_1$ are mutually inclined at an angle of 43½′.

The same results will follow, perhaps more simply, from

a study of the stereographic projection of fig. 1; for as $\mathrm{T}c = \mathrm{T}\delta$, c_1 will be rotated into the position of δ, and δ_1 into the position of c, while γ_1 will be rotated to d, and d_1 to γ. With the other poles it will be different: thus α will rotate into the position α_1, where $\mathrm{T}\alpha = \mathrm{T}\alpha_1 = 89^\circ\ 18\frac{1}{4}'$, and $a\alpha_1 = \mathrm{T}a - \mathrm{T}\alpha_1 = 90^\circ 41\frac{3}{4}' - 89^\circ 18\frac{1}{4}' = 1^\circ 23\frac{1}{2}' = \beta b_1 = \alpha a_1 = b\beta_1$. Also $\mathrm{T}\,\overline{\mathrm{Q}}_1 = \mathrm{T}\,\overline{\mathrm{Q}} = 89^\circ\ 9'$, whence $\mathrm{Q}\,\overline{\mathrm{Q}}_1 = 90^\circ\ 51' - 89^\circ\ 9' = 1^\circ\ 42'$; and $\mathrm{T}\,\overline{\mathrm{C}} = \mathrm{T}\,\overline{\mathrm{C}}_1 = 44^\circ\ 34\frac{1}{2}'$, whence $\mathrm{C}\,\overline{\mathrm{C}}_1 = 90^\circ\ 51'$.

Next, let M be a point bisecting the arc $\mathrm{Q}\,\overline{\mathrm{Q}}_1$; the poles of the two octahedra will be symmetrically disposed to the twin-plane, represented in the projection by the line $\overline{\mathrm{M}}\,\mathrm{B}\,\mathrm{M}$; for $\mathrm{T}\,\mathrm{M} = \mathrm{T}\,\mathrm{B} = 90^\circ$, and the arcs $\mathrm{T}b_1$ $\mathrm{T}\alpha_1$ $\mathrm{T}\alpha$ $\mathrm{T}b$ $\overline{\mathrm{T}}a$ $\overline{\mathrm{T}}\beta$ $\overline{\mathrm{T}}a_1$ $\overline{\mathrm{T}}\beta_1$ are all equal. From this it will follow that $\mathrm{M}a = \mathrm{M}\beta = \mathrm{M}\alpha_1 = \mathrm{M}b_1$; also that $\mathrm{M}a_1 = \mathrm{M}\beta_1 = \mathrm{M}b = \mathrm{M}\alpha$, and that $\mathrm{M}\gamma$ $\mathrm{M}\gamma_1$ $\mathrm{M}d$ $\mathrm{M}d_1$ $\mathrm{M}c$ $\mathrm{M}c_1$ $\mathrm{M}\delta$ $\mathrm{M}\delta_1$, being all right angles, are equal to each other. The poles of the two individuals are thus not only symmetrical to the twin-plane $\mathrm{M}\,\mathrm{B}\,\overline{\mathrm{M}}$, but also to the plane $\mathrm{T}\,\mathrm{B}\,\overline{\mathrm{T}}$ at right angles with it, represented by an irrational symbol approximating to (100 0 99), and thus not a crystalloid plane. The same arrangement of poles might therefore be obtained by rotating the original octahedron about the line $\mathrm{M}\,\overline{\mathrm{M}}$ parallel to the tangent of the circle at T, and therefore to a "terminal edge" of the octahedron {1 1 1}. It may be remarked that, although the same arrangement of poles will be obtained by rotation about the lines $\mathrm{T}\,\overline{\mathrm{T}}$ $\mathrm{M}\,\overline{\mathrm{M}}$, the lettering will not be identical in the two cases; but as in both cases the planes which are quite or nearly coincident in direction are always represented in the one individual by italic letters and in the other by greek, there will be no crystallographic difference in the results of these two methods of derivation. This proves that, as has been mentioned above, Haidinger's statement, "regular composition often takes place parallel to a plane of {1 0 1} or perpendicular to the terminal edges of {1 1 1}," indicates only a single case if the reference be to the plane of rotation.

Haidinger's law might therefore be equally well expressed in the two following ways:—

I. Twin-plane a face of the form {1 0 1}; composition-plane perpendicular to the twin-plane.

II. Twin-axis a terminal edge of {1 1 1}; composition-plane parallel to the twin-plane.

We are now in a position to discuss the difference of growth which will be produced by a variation in the position of the plane of composition. As the line $\mathrm{T}\,\overline{\mathrm{T}}$ is perpendicular to the

edges $A\bar{C}$ $C\bar{A}$ and passes through the centre of the crystal, the rhombs $TB\bar{T}\bar{B}$ $TB_1\bar{T}\bar{B}_1$ of figs. 2 and 3 will be perpendicular to the twin-plane, and will each represent the composition-plane of Haidinger. If, now, by simple translation without rotation the half $TB\bar{T}\bar{B}AC$ above the rhomb of fig. 2 be associated with the half $T\bar{B}_1\bar{T}B_1\,A_1C_1$ below the rhomb of fig. 4 in such a way that the two rhombs coincide, we get the composition represented in fig. 5. This figure will therefore be that of a growth according to the law of Haidinger. For convenience of direct comparison with the results following from the law as given by Naumann and Sadebeck, the same growth as fig. 5 is shown in fig. 6 as it would be seen either after a rotation of the whole figure through two right angles about the vertical axis, or by an eye placed at the back of the paper.

Again, let the rhombs $MB\bar{M}\bar{B}$ $MB_1\bar{M}\,\bar{B}_1$ of figs. 2 and 3 be the traces on the octahedron-faces of a plane parallel to the twin-plane, and therefore the composition-plane of Naumann. If now, just as before, the half $MB\bar{M}\bar{B}C\bar{A}$ above the rhomb of fig. 2 be associated with the half $\bar{M}\bar{B}_1\,MB_1\,A_1\bar{C}_1$ below the rhomb of fig. 4 in such a way that the rhombs coincide, we get the composition represented in fig. 7. This figure will therefore be that of a growth according to the law of Naumann.

With the help of the stereographic projection of fig. 1 we can now investigate the differences of the growths represented respectively in figs. 6 and 7.

Fig. 6.

Haidinger's law.

$$\left.\begin{aligned} d\gamma_1 &= \gamma d_1 = 0, \\ c\delta_1 &= \delta c_1 = 0. \end{aligned}\right\}$$

Edges $C\bar{T}$ $\bar{T}C_1$ coincident,
,, AT TA_1 ,,

Angle between the truncating planes of C and C_1 $= 89°\ 9'$.

The faces $a\beta a_1\beta_1$ are not in a zone.

Fig. 7.

Naumann's law.

$$\left\{\begin{aligned} a\alpha_1 &= \beta b_1 = \text{salient } 1°\ 23\tfrac{1}{2}', \\ b\beta_1 &= \alpha a_1 = \text{reentrant } 1°\ 23\tfrac{1}{2}'. \end{aligned}\right.$$

Angle $CM.M\bar{C}_1$ = salient 1° 42′,
,, $\bar{A}\bar{M}.\bar{M}A_1$ = reentrant 1° 42′.

Angle between the truncating planes of C and $\bar{C}_1$ $= 90°\ 51'$.

The faces $d\gamma c_1\delta_1$ are in a zone.

We have seen that in the projection the plane of composition is a plane of symmetry to the poles of the two individuals; and we further perceive that in each case the plane of composition is a plane of symmetry to the faces actually shown by

the twin-growths; and the angles of the upper half of fig. 6 are exactly equal to the similarly disposed angles of the upper half of fig. 7, and the angles of the lower half of fig. 6 to the similarly disposed angles of the lower half of fig. 7, the only difference in the growths being the relation of the upper to the lower half.

Of the specimens of copper pyrites in this collection, one figured by Haidinger himself in the 'Catalogue of the Allan-Greg Collection' (now in the possession of the British Museum), though probably not one of the original specimens of the memoir of 1822, offered itself as the most likely to afford a satisfactory solution of the difficulty. In this specimen, which comes from Freiberg, the twin-growths are disposed parallel to each other on galena, and are associated with quartz, chalybite, and calcite. The length of a side of the triangular faces is about 1·5 millim. Haidinger's drawing to illustrate this specimen is virtually the same as fig. 9 (copied from his paper of 1825), the only difference being that the faces of $e\{1\,0\,1\}$ being almost linear are not shown in the Catalogue. This figure represents the rotation as taking place about each of the normals to the four upper faces of $\{1\,0\,1\}$ of the central individual, of which both the upper and the lower halves are present. We may remark that a complex growth of this perfect kind would be explained by the law of Naumann equally with that of Haidinger, seeing that in the lower half of the regular composition the various planes of junction are represented as parallel to the corresponding twin-planes, and in the upper half as perpendicular to them. As none of the growths have an all-round development, the figure represents the growth in theoretical perfection rather than as actually existent; in fact the actual habit is more nearly shown in fig. 10, which at the same time will serve to give an idea of the striation to be observed on the faces.

A crystal from this specimen appeared to show that the triad of octahedron-faces $o\,o_1\,o_2$ did not quite coincide, as according to Haidinger's explanation should be the case; but still no satisfactory measurement of the angle could be obtained. The reentrant angle lying in the zone $c\,c_1$ at the junction of two individuals could, however, be determined with very fair precision, although the faces are finely striated parallel to their edge of intersection with each other. These planes were supposed by Haidinger to belong to the common form $z\{2\,0\,1\}$; and as the angle made by a plane of this octahedron with the adjacent face of the form $\{1\,0\,1\}$ is 18° 31′, the reentrant angle according to his theory should be 37° 2′. Actual measurement,

however, gave 20° 9′; and closer examination rendered it clear that on the crystal measured the faces belonged not to the form $z\{2\,0\,1\}$, but to another less common form $h\{3\,0\,2\}$, whilst smaller almost linear faces of $z\{2\,0\,1\}$ were to be seen lower down in the reentrant angle. The angle between (3 0 2) and (1 0 1) being 11° 20½′, the reentrant angle for this form should be, according to Haidinger's law, 22° 41′; on the other hand, according to Naumann's law, it should be 20° 59′—that is, the difference of the inclinations of (3 0 2) and its parallel $(\bar{3}\,0\,\bar{2})$ to the plane $(1\,0\,\bar{1})$. The measured angle was thus 50′ less than the angle calculated from Naumann's law; and the latter angle was itself 1° 42′ less than the one calculated according to that of Haidinger.

The difference between the calculated and measured angles is large, and in fact is so considerable that it can only be attributed either to the growth being not strictly a regular twin, or to a deviation from the fundamental angle as determined by Haidinger. As the extreme accuracy of Haidinger's measurement of the fundamental angle of copper pyrites had been confirmed both in the memoir of Sadebeck and in the Catalogue of the Strassburg collection, and also by Kokscharow*, and as, further, this particular crystal did not lend itself to a precise determination of any other angle than the reentrant one above mentioned, the result seemed very unsatisfactory, and for some time the examination was discontinued. Two years later it was resumed; and, happily, another crystal from the same specimen was found to give reflections so good that a precise measurement of the angle between two octahedron-planes belonging to the same individual and on opposite sides of c could be obtained. This was found to be 108° 17½′, a deviation of 22½′ from the angle as determined from the specimens previously measured. A less precise determination of the angle of a terminal edge gave as mean 69° 59¼′, the limiting values being 69° 54¾′ and 70° 0½′. From the same crystal the angle $o\,o_1$ was found by help of the δ eyepiece of a Fuess's goniometer (as improved by Websky) to be 2° 3′, instead of zero according to Haidinger, and 1° 23½′ according to Naumann and Sadebeck.

The whole difficulty had, however, now disappeared, as is shown by the following table of calculated and observed angles:—

* *Bull. Soc. St. Pét.* 1874, xix. p. 562.

If 001.101=	44° 34½′	44° 22½′	44° 22′	44° 21½′	Observed.
	Calculated.				
$\omega_2\,\omega'_2$	108° 40′	108° 17¼′	108° 16¼′	108° 15¼′	108° 17½′
$o\,\omega$	70 7½	69 56	69 55½	69 55	69° 54¾′–70° 0½′, mean 69° 59¼′
$o\,o_1$ Naum.	1 23½	2 3	2 4½	2 6	2° 3′
$o\,o_1$ Haid.	0 0	0 0	0 0	0 0	
$h\,h_1$ Naum.	20 59	20 12¾	20 10¾	20 8¾	20° 9′
$h\,h_1$ Haid.	22 41	22 42¾	22 42¾	22 42¾	

From a third crystal not quite so perfect, but still giving very good images, the angle $\omega_2\,\omega'_2$ was determined to be 108° 18¾′.

There can thus be no doubt that:—

1st. The growth is strictly regular.

2nd. The parametral angle differs from that determined from other specimens by previous observers, and is very nearly 44° 22′.

3rd. The twin-plane is a face of the form {1 0 1}. And

4th. The composition-plane is *parallel* (and not perpendicular) to the twin-plane.

At the suggestion of Prof. Maskelyne, a careful analysis of this specimen was made in the departmental laboratory, with the view of ascertaining to what extent this variation in the fundamental angle is attended by a difference from the chemical composition of ordinary copper pyrites. The following results were obtained by Dr. Walter Flight:—

	Observed. I.	Observed. II.	Calculated, $CuFeS_2$.
Copper	25·78	30·66	34·45
Iron	35·16	34·11	30·57
Sulphur	37·52	[35·23 by diff.]	34·98
Arsenic	traces		
Quartz	0·28		
	98·74	100·00	100·00

No other metals were discovered, although carefully searched for. The specimen is thus found to contain a considerable and variable excess of FeS_2 over that of typical copper pyrites—the first analysis corresponding very nearly to $CuFeS_2 + \frac{1}{2}FeS_2$, and the second to $CuFeS_2 + \frac{1}{4}FeS_2$. As close examination of the specimen reveals the presence of included minute crystals of iron pyrites (and mispickel), it is possible that even in the second fragment, which was specially

chosen for its approximate homogeneity, this excess of FeS_2 may be almost wholly due to mere admixture of iron pyrites.

Since the above determinations were made, the collection has been enriched by the acquisition of a superb Freiberg specimen of very much the same character as the one just described, but on a relatively colossal scale; for the length of side of the triangular face $o\,o_1\,o_2$ (and this is the smallest) is here not 1·5 but 24 millim. Owing to the absence of reentrant angles, the growth very much resembles a regular octahedron truncated by the faces of a cube, one of the octahedron-faces, however, showing in a very marked way the threefold composition of that part of the crystal.

A further examination of the collection has resulted in the finding of a specimen (from Pool mines near Redruth) which confirms the parallelism of the planes of composition and of twinning in the most satisfactory way. The crystals of copper pyrites have been here deposited on quartz crystals which they partly enclose: they thus have not an all-round development; in fact, there is a practical difficulty in determining more of the growth than is shown in fig. 11; but this, so far as it goes, falls little short of perfection (the dotted lines indicate the twin in theoretical completeness). The images from $\omega\,\omega_2$ were so well defined that the largest magnifier, α, of Fuess's instrument could be used, three different measurements at nearly normal incidence giving respectively for the value of this angle 1° 23′, 1° 23½′, and 1° 23′, while a measurement at almost grazing incidence, when the images were broader, gave 1° 23½′. The faces $o\,o_2$ were not so perfect; but still, with the δ eyepiece, good images were seen, from which two consecutive measurements of the angle gave 1° 23′ and 1° 23½′. Two measurements of $o\,o'$ gave respectively 108° 39′ 30″ and 108° 39′ 45″. The following table renders more evident the close correspondence of the observed and calculated angles.

Observed. Angles of one individual.		Observed. Symmetrically disposed angles of the other.		Calculated from 001.101 =44° 34½′.
$o\,\omega$	70° 7½′	$o_2\,\omega_2$	70° 7½′	70° 7′ 32″
$\omega\,e$	35° 3½′	$\omega_2\,e_2$	35° 4′	35° 3′ 46″
$\omega\,o'$	70° 4¾′	$\omega_2\,o'_2$	70° 12′, 70° 4½′	70° 7′ 32″
		$o'_2\,e'_2$	35° 4′	35° 3′ 46″
$o\,o'$	108° 39⅝′	$o_2\,o'_2$	108° 37′	108° 40′ 4″
Also $\omega\,\omega_2$	1° 23′, 1° 23½′, 1° 23′, 1° 23½′			1° 23′ 30″
$o\,o_2$	1° 23′, 1° 23½′			1° 23′ 30″

The linear faces $e\{1\,0\,1\}$ and the minute faces of another

octahedron $g\{2\,0\,3\}$, although present on the crystal, are not shown in the figure.

A chemical examination made by Dr. Flight shows that this specimen has a composition very nearly represented by the typical formula $CuFeS_2$. The following results were obtained:—

	Observed.	Calculated, $CuFeS_2$.
Copper	34·37	34·45
Iron	30·03	30·57
Sulphur.........	31·92	34·98
Quartz	4·19	
	100·51	100·00

Up to this point, for the sake of simplicity, a very important property of copper pyrites, its hemihedral structure, has been left as much as possible out of sight. It is found, however, that the faces of the octahedron $\{1\,1\,1\}$ of this mineral are not all similar, but must be regarded as belonging to two distinct tetrahedra: in the case of the first tetrahedron, for convenience of distinction termed the positive or o tetrahedron, the faces are rough or striated, and are sometimes coated with oxide of iron; on the other hand, the faces of the second or negative or ω tetrahedron are smooth, bright, free from this coating, and in general smaller than the former. From this it follows that the set of faces denoted above by the italic letters *a b c d*, though similar to each other, are distinct in physical character from those denoted by the greek letters $\alpha\,\beta\,\gamma\,\delta$; whence we infer that in such a growth as would be represented by fig. 7, where there has been a simple rotation of one individual through two right angles from a position of identical orientation with the other, and adjacent faces of the two individuals thus bear respectively italic and greek letters, the composition-plane will be a plane of geometrical, but not of physical symmetry. As, however, the correlative tetrahedra and also the correlative hemiscalenohedra are independent of each other, not only in surface-characteristics, but also in their presence on the crystal, even this geometrical symmetry could scarcely be expected in the actual twin-growth.

Now Sadebeck states that in the actual twin-growth the composition-plane is really a plane of symmetry not only to the geometrical, but to the physical peculiarities—the regular composition thus belonging to the class called by Groth "symmetric twins;" that instead of the faces of the octahedron which are parallel, or nearly so, in the two individuals belonging in one to the positive, and in the other to the negative, they really belong either both to the positive or both to the negative tetrahedron. To pass, therefore, to the actual

twin from parallel orientation of the individuals, there must be, in addition to the rotation through two right angles round a normal to $(1\,0\,\bar{1})$, a *further rotation* of one of the two crystals either through two right angles about a normal to one of the faces of the prism $\{\bar{1}\,1\,0\}$, or through a single right angle about the vertical axis parallel to the edges of this prism. Though this double rotation may be compounded into a single rotation round the normal to a face of the octahedron $\{1\,1\,\bar{1}\}$, the angle of this single rotation will not be 180°, as is the case in other twins, but $\bar{1}19°\ 31'$.

If this statement of Sadebeck be accepted as having a satisfactory foundation, the growth must be regarded as up to the present unique in character; for no other regular composition appears to have yet been discovered in which, starting from a parallel orientation, a double rotation is *absolutely* necessary for the representation of the relative disposition of the two individuals. There exist twin-growths of *tetartohedral* crystals, it is true, such as those of sodium chlorate and certain regular compositions of quartz, described by Prof. Groth, which are somewhat analogous in character; but they are capable of a more or less satisfactory representation by a simple rotation of one of the individuals through two right angles from a position where corresponding crystallographic lines of the right and left individuals are identical in direction; they are moreover intimately related to the directions of the crystallographic axes. As, however, it had been impossible for Sadebeck to convince himself, from simple examination of the specimens, that certain faces assumed by Haidinger to be parallel might not be inclined to each other at an angle of $1°\ 23\frac{1}{2}'$, it was possible to entertain a doubt as to the specimens being sufficiently well crystallised to allow of an absolute certainty in the distinction of the two tetrahedra; and as the law is so curious from its extreme rarity and simplicity, and so important in its bearing on the general question of twin-growth, about which there has lately been much discussion, it seemed desirable to place the law, if possible, beyond all suspicion.

The accuracy of Sadebeck's inference as to the disposition of the two tetrahedra in this twin-growth is confirmed in the most satisfactory manner by the specimens in this collection.

The Freiberg specimen of fig. 10 shows not only that the individuals are symmetrical to the plane of composition, but also that the differences of the two tetrahedra of each individual are too marked to allow of this symmetry of physical peculiarities being an accident of the growth.

The specimen from Pool mines (fig. 11) is even more satisfactory still; for the faces $\omega\,\omega_2$ which give such excellent

images are perfectly smooth and bright, and remarkably different in aspect from the two dull and striated faces $o\ o_2$.

A further example is presented by a specimen (probably from the Trevannance mine, St. Agnes) shown in fig. 12, which the symmetry to the combination-plane and the extreme difference between the smooth and the deeply-striated tetrahedra render most convincing. The angle between these striations is so very definite that it can be measured with fair accuracy by means of a microscope; it was determined to be $120\frac{3}{4}°$, the angle calculated according to Naumann's law being $120° 28'$, and according to Haidinger's law $119° 31'$.

Finally, we may refer to fig. 8, representing a Cornwall specimen (now in the Museum) figured in 1825 by Haidinger himself in his memoir on the Regular Composition of Crystallised Bodies. Here the predominant form of each individual is a hemiscalenohedron; and this in each pair is symmetrically disposed to the plane of composition. Although this specimen is symmetrical in its habit, the planes *s* are so striated and rounded that it was found impossible to assign to them a definite symbol; they lie, however, in the zone defined by the symbol $[1\,1\,\bar{2}]$, and approximate to $\{3\,1\,2\}$.

We conclude, therefore, that there is no doubt of the actual existence of a kind of twin-growth which it is not possible to represent by a single rotation through two right angles from a position of parallel orientation of one of the individuals to the other—that for the representation of this growth an additional rotation is requisite, but that the simplest mode of representation is the one which regards the two individuals as symmetrical to a plane.

EXPLANATION OF PLATE VI.

(To accentuate the differences in twin-growths according to the laws of Haidinger and Naumann, figs. 1–7 are drawn for a parametral angle $42\frac{1}{2}°$ instead of $44° 34\frac{1}{2}'$.)

Fig. 1. Stereographic projection of the poles of $\{1\,1\,1\}$, and of the same twinned about $T\,\bar{T}$, the normal to $(1\,0\,\bar{1})$.

Fig. 2. The octahedron $a\,b\,c\,d\,\alpha\,\beta\,\gamma\,\delta$ $\{1\,1\,1\}$.

Fig. 3. The octahedron $a_1\,b_1\,c_1\,d_1\,\alpha_1\,\beta_1\,\gamma_1\,\delta_1$ $\{1\,1\,1\}$, parallel to the last.

Fig. 4. The same turned through two right angles round $T\,\bar{T}$ the normal to $(1\,0\,\bar{1})$.

Fig. 5. Twin-growth of $\{1\,1\,1\}$, according to Haidinger's law.

Fig. 6. The same, viewed from the opposite side.

Fig. 7. Twin-growth of $\{1\,1\,1\}$, according to Naumann's law.

Fig. 8. Twin-growth, with faces *s* of a hemiscalenohedron or disphenoid (Haidinger, Edin. J. of Sc. 1825).

Fig. 9. Twin-growth of $\{1\,1\,1\}$ $\{0\,0\,1\}$ $\{1\,0\,1\}$ $\{2\,0\,1\}$ (Haidinger, Edin. J. of Sc. 1825).

Fig. 10. A similar twin-growth.

Fig. 11. A twin-crystal from Pool mines, near Redruth.

Fig. 12. A twin-crystal, probably from Trevannance mine, St. Agnes.

XXXII. *The Tails of Comets.* *By* E. Vansittart Neale.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I venture to offer an explanation of the remarkable phenomena presented by the tails of comets, so simple that, considering the number of eminent astronomers who have turned their attention to this subject, and to whom it has remained a mystery, I am almost afraid to suggest my explanation, lest it should prove a sort of scientific mare's nest. Still, since I cannot see where it fails, while the subject is one of considerable interest, I have determined to run the risk of some eye more penetrating than my own discovering my error, if there is one.

My explanation rests on the interaction of three forces, of which two are known to exist, while the existence of the third may, I think, be reasonably assumed. These forces are:—(1) the force which urges the comet towards the sun; (2) the expansive force of the heat of the sun; (3) the resistance of an atmosphere surrounding the sun.

That there is such an atmosphere extending many hundred thousand miles from the sun's centre we know, because it can be seen. How much further it may extend in a condition in which it is invisible we do not know; but, considering the distance to which the atmosphere surrounding the earth is known to extend by its action on meteoric bodies, we may, I think, reasonably assume the existence of an invisible solar atmosphere, extending far beyond the limits of the visible atmosphere; and if this is assumed, we obtain an easy explanation of the phenomena of comets' tails.

The changes in a comet which give rise to its tail begin, according to the account given by Mr. Higgins in his excellent article on Comets in the current number of the 'Nineteenth Century,' by jets of a gaseous nature ejected towards the sun, *i. e.* in the direction determined by the expansive force of the sun's heat, acting on the comet, combined with the moving force of the comet. But soon, he says, these jets bend round, as if they were carried back by a strong wind, and form an envelope round the body of the comet and a cone behind it. Just so. It is what would happen to a man dressed in a loose robe and running rapidly through the air. His dress, though moving with him, would stream behind him, because it experienced more resistance than his body from the air. The jets

of gas which escape from the body of the comet and expand when they leave it are more resisted by the sun's atmosphere than its more solid head, which therefore gradually overtakes them ; so that they seem to fall back, till they constitute an envelope round it, and then spread in a conical form behind the head, through the joint operation of their own lateral movements, of the increasing expansive force of the sun's heat as the comet approaches the sun, of the increasing resistance of the sun's atmosphere (whence the head must continually gain more and more on the parts of the tail at first thrown off), and of the pressure of fresh envelopes continually forming round the nucleus as it advances. The body of the comet is constantly moving through the gases or vapours which it throws off, and thus produces the appearance of a tail, by leaving each successive part thrown off more and more behind it.

The central line of these successive envelopes would obviously tend to be a straight line from the sun's centre through the head of the comet; though the continual change of direction in this line, as the comet approaches its perihelion, must be liable to produce an apparent curvature in the tail, because the parts first emitted, and therefore most distant from the body, if they retain luminosity enough to be visible, must fall more and more behind the advance of this central line in its sweep round the sun.

The phenomena of divided tails, of bright streaks, &c., find a ready explanation in the accidental variety of pressures to be expected among jets of gas or vapour emitted under such circumstances, and the effect of perspective, according as we happen to look through the edges or across the more central parts of the envelopes forming the comet's tail—possibly combined with actual variations of pressure in the sun's invisible atmosphere, arising out of the enormous changes which can be observed in its visible atmosphere. But what as to the change in the direction of the tail when a comet has passed its perihelion? Why do comets then carry their tails before instead of behind their heads ? Because the direction of the pressures which produce the tail has changed. Given an invisible solar atmosphere, a comet moving towards the sun will be perpetually passing from a rarer into a denser medium, while a comet moving from the sun will be perpetually passing from a denser into a rarer medium. At the same time the jets of gases or vapours which it will continue to emit from the expansive force of the sun's heat will then consist of particles moving from the sun. Thus the two tendencies—the movement of these particles due to the action of gravity, and the tendency of the expansive force to exert itself in the line of

least resistance—will combine to carry the luminous particles emitted from the comet in advance of the mass.

That the change in the direction of a comet's tail should take place with the rapidity and to the extent observed in the case of the enormous appendages of some of these bodies may still appear surprising. But it must be remembered that, in these cases, we can speak only of what we see. The conical mass of gases or vapours extending behind the nucleus of a comet may attain, in the case of the largest of these bodies, to an expansion much greater than the part visible, which may consist only of the parts that receive the strongest impulses from the centre of force; so that when the tail seems to have swung round through an enormous arc in the sky, what has really happened may be only that the line along which the substances forming it become visible may have shifted, in consequence of the direction of the impulses proceeding from the head having altered.

As the time when the most rapid alteration in the direction of the tail of a comet takes place necessarily coincides with that when the expansive action of the sun on the substances emitted from the comet is at its maximum, there must be the less difficulty in admitting the last hypothesis as an explanation of this phenomenon. It is the only one, so far as I see, that offers any difficulty in the way of the theory now proposed respecting the tails of comets, which may be summed up in the proposition that, as the incandescence of meteoric bodies proves to us the existence of a widely diffused atmosphere surrounding the earth, so the development of the tails of Comets proves to us the existence of a much more widely diffused atmosphere surrounding the sun—both sets of phenomena being due to the same cause, namely the resistance of these atmospheres to bodies rapidly passing through them.

I am yours &c.,

E. Vansittart Neale.

15 Portsmouth Street, Manchester,
August 21, 1882.

XXXIII. *Simple Method for Calibrating Thermometers.* *By* Silas W. Holman*.

THE calibration of a thermometer by most of the methods in ordinary use is a tedious and somewhat difficult operation, and hence often neglected even in important work. For the purpose of supplying a method simple both in observation and computation, and at the same time accurate, the following

* From Silliman's American Journal, No. 136, p. 278.

process is described, which, although involving little that is novel, has not to my knowledge been used before.

First, however, it is necessary to recall to the attention of observers the fact that, without calibration-correction, the readings of a thermometer having a scale of equal linear parts cannot be relied upon within one or more divisions of this scale, and that thermometer-makers, knowing this, almost universally space the graduation upon the tube to correspond more or less closely with the shape of the bore, as determined by previous calibration, or by comparison with a standard (!) instrument. This practice is much more general than is ordinarily supposed, and has an important bearing upon the accuracy of the work done with such instruments. For the scale thus made is merely approximate, the dividing-engine or other tool being usually changed only at such intervals as to make the average error less than some specified amount. An inspection of these conditions will show that the calibration of such a tube and scale can be only approximate, except with corrections for the inequalities of the spacing, involving an amount of labour disproportionate to the result obtained. The best makers, such as Fastré, Baudin, and others, have produced satisfactory thermometers graduated to equal volumes; but even these are not as reliable as instruments of less cost with a scale of equal linear parts, say of millimetres, supplemented by a calibration by the observer. The best form of tube for almost all work is one backed with white enamel, with an inverted pear-shaped bulb at the upper end of the capillary (a very important feature), and with a scale of equal arbitrary linear parts (0·7 to 1 millim. is a suitable length for estimation of tenths) or of approximate degrees, for convenience, etched or engraved upon it.

Without reviewing here the methods proposed by various writers, it may be said that it has been the general plan to select beforehand upon the scale two points between which to make the calibration, this space being the "calibration unit," the errors of these points being, of course, zero. This plan has led to unnecessary complexity. Such an assumption is no more requisite in calibration after a scale has been put upon the tube, than in calibrating by the dividing-engine or micrometer before making the scale. It is obvious that the selection of these points is wholly arbitrary, and, if used at all, one or both of them may, if desirable, be chosen after the observations with the calibrating-thread have been made. The choice should be made with a view to facilitating the work. Hence the use of the observed freezing- and boiling-points, upon which some methods are based, is most undesirable.

In the method which will now be given, either one or both of these points may be left to be selected, according to the combined conditions of length of thread employed, shape of the tube, and numerical convenience, after the observations with the thread have been made.

Let it be desired to find the calibration-corrections for a given tube. Determinations which will give the errors of every 3 centim. of length will ordinarily be sufficient; but this must depend on the result sought. Separate a thread of mercury of about that length. The actual length of the thread within two or three millimetres is of no consequence whatever; and hence a suitable thread can be obtained in a very short time.

Set the thread with its lower end at or near the beginning of the graduation: call the reading* of the lower end of the thread l_1, and that of the upper end u_1. Move the thread less than 1 millim. and read again, finding thus l_2 and u_2. Move the thread about 1 centim., and read l_3 and u_3. Move the thread less than 1 millim., and read l_4 and u_4. So continue throughout the whole length of graduation, increasing the number of settings or repeating the whole series in reverse order and several times, if the highest attainable precision is desired. This alternation between 1 millim. and 1 centim. in setting tends towards the better elimination of errors in estimation. It is not, however, essential, nor even always as well as an equal number of distributed readings. This must depend upon the skill of the observer. Avoid, as far as convenient, taking readings with an end of the thread apparently just at the line of the scale, as the width of the line, even in the best scales, is a source of considerable error†. If any point (*e. g.* the zero-point of the graduation) has for any reason been selected as the first of which the error should be assumed zero, the settings may to advantage, though not necessarily, be made to extend each way from this.

Then u_1-l_1, u_2-l_2, &c. will give a series of lengths of the calibrating-thread in all parts of the tube. Before reuniting this thread to the rest of the mercury, plot points with abscissas l_1, l_2, &c., and ordinates u_1-l_1, u_2-l_2, &c., the corresponding lengths of thread, and draw a smooth curve through the points thus obtained. This line will give a general idea of the form of the capillary bore; and should any parts of it show considerable irregularities, the corresponding portions of the tube should at once be reexplored with the thread.

If not already done, the point A upon the scale, to be used

* Tenths of a division are supposed to be read by estimation.

† Some of the advantages of Neumann's method are offset by this error.

as the starting- or reference-point of the computation, should now be selected. In general the extreme ends of the tube are to be avoided, as more likely to have been rendered irregular or rapidly tapering in the process of making or joining on the bulbs. If the zero of the numbering is placed one or two centimetres from the bottom of the tube, it forms a desirable starting-point.

Find upon the curve the ordinate u' corresponding to the abscissa A; then with abscissa $A+u'$ find the corresponding ordinate u'', with abscissa $A+u'+u''$ find the corresponding ordinate u''', continuing to the upper limit of the graduation. If A is at a sufficient distance from the lower end of the graduation, find a similar series below the point A. These points, A, $A+u'$, $A+u'+u''$, &c., upon the graduation are separated by equal volumes of the capillary. Select any one of these as the second point of which the error is to be arbitrarily assumed as zero, and call this B. Then

$$A+u'+u''+\ldots+u\,\text{nth}=B.$$

There are thus n spaces of equal volume between A and B; and these correspond each to $\frac{1}{n}$th of the interval $B-A$. Hence the true reading (which, however, it is not necessary to compute numerically) at the point

$$\begin{array}{ll} A & \text{is } A, \\ A+u' & \text{is } A+\frac{1}{n}(B-A), \\ A+u'+u'' & \text{is } A+\frac{2}{n}(B-A), \\ \ldots & \ldots \\ B & \text{is } B. \end{array}$$

And the error obtained by subtracting the true readings, as given in the right-hand column, from the corresponding actual readings, given in the left-hand column, at

$$\begin{array}{ll} A & \text{is } 0, \\ A+u' & \text{is } A+u'\left\{A+\frac{1}{n}(B-A)\right\}=u'-\frac{1}{n}(B-A), \\ A+u'+u'' & \text{is } u'u''-+\frac{2}{n}(B-A), \\ \ldots & \ldots \\ B & \text{is } 0. \end{array}$$

In selecting B it might have been assumed equal to $A+u'$, thus making $n=1$. This would somewhat simplify the calcu-

lation, and would be of equal accuracy, but is objectionable from the fact that, in general, this volume would differ considerably from the average volume obtained when n has a greater value (always an integer), and the resulting series of errors would assume larger numerical values.

The errors or corrections are, for purposes of interpolation, most conveniently represented graphically by a smooth curve through points with abscissas proportional to the direct readings

$$A, \; A+u', \; A+u'+u'', \; \&c.,$$

and ordinates to the corresponding corrections.

Should it be necessary to increase the accuracy by a second calibration with a thread of different length, it is only necessary to take one of approximately an integral part of $(B-A)$, and when the final curve of error is drawn make the error at B equal to zero, distributing the difference at that point proportionally to the scale-readings among the errors at the intermediate points—in other words, to shift the axis of the second curve of error so that it shall make the error at B zero.

This method requires for each calibration the use of but a single thread. The computation is simple, and involves a minimum of approximation. Errors of observation are largely eliminated by the number of settings made in all parts of the tube, and by the inspection of the curve of lengths; both of which operations tend in an unusual degree to detect mistakes or any minor irregularities of the capillary. It avoids the common requirements of setting the thread exactly at certain definite points in the tube, or any approximate correction for slight errors in such setting—two sources of considerable error and inconvenience, especially when the thread must be set near or under a line of the graduation. And, lastly, the total time of calibration for a result of given accuracy is reduced to one half or one third of that required by Neumann's method, the quickest and most satisfactory with which I am acquainted except that given by Pickering. The latter, described with some slight inaccuracies, at the reference noted below, is a neat application of the graphical method; and the curve of lengths of thread adopted in the method which I have described is identical with the corresponding one given by Professor Pickering, while the whole process is fully one third shorter and somewhat more accurate. From a series of calibrations executed upon the same thermometer (one with a millimetre-scale, by Baudin, of Paris), using a variety of methods, I have obtained slightly more concordant results with the proposed method than with Neumann's or Pickering's

(all those possessing, however, nearly the same degree of precision), and decidedly better results with these than with any of the other existing simple methods.

Considerable aid in eliminating errors of parallax in such work is sometimes found by looking down upon the horizontal thermometer through a vertical tube having a small hole at each end. One of the cheap French microscopes with its lenses removed, and inverted in its stand, answers this purpose well.

With such a device two calibrations of the above-described thermometer with threads of 3 and 5 centim. respectively, each with only one series of observations, and requiring not more than one hour and a half each for completion, gave results whose average difference from each other at nine points was 0·04 millim., and the arithmetical sum of the extreme differences was 0·12 millim., a result of sufficient accuracy for any class of work of which such an instrument is capable.

For brief descriptions of methods of separating threads of mercury for calibration, reference may be made to the paper by Russell, and the text-book by Pickering, noted below. These processes are in general use, and are safe and convenient.

References upon Calibration of Closed Thermometer-Tubes.

Bessel, Pogg. *Ann.* vi. p. 287 (1826).
Rudberg, Pogg. *Ann.* ix. pp. 353, 566; xxxvii. p. 376 (1836); xl. pp. 39, 562 (1837).
Kohlrausch, 'Physical Measurements,' p. 59 (English translation).
Pickering, 'Physical Manipulation,' ii. p. 75 (1876).
Thiesen (Neumann's Meth.), Carl's *Rep.* xv. p. 285 (1879).
Russell (Neumann's Meth., transl. from Thiesen), Amer. Journ. Sci. xxi. p. 373 (1881).
Marek, Carl's *Repertorium,* xv. p. 300 (1879). (Solution by least squares.)
von Oettingen, *Inaug. Diss.*, Dorpat, 1865. (This I have been unable to obtain.—S. W. H.)

XXXIV. *On* Boltzmann's *Theorem on the average Distribution of Energy in a System of Material Points*.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Gräz, April 6, 1882.

POSSIBLY you may feel disposed to have the accompanying notice of a paper of Maxwell's translated for your valuable 'Philosophical Magazine.' So far as I know, this excellent paper of Maxwell's has not been reprinted in your Magazine; it may not, therefore, be without interest to your readers that some notice of it should appear, if only as

* Translated from Wiedemann's *Beiblätter.*

an abstract; and, further, my notice contains one on two new things, amongst which a remark on Watson's excellent book may be interesting to English physicists.

With highest esteem,

Yours &c.,

BOLTZMANN.

MAXWELL (Camb. Phil. Trans. vol. xii. part 3, pp. 547–570, 1879) shows that this theorem may be easily proved by means of Hamilton's principle. The theorem is also extended, since it is shown to hold good for any systems determined by generalized coordinates, if only they satisfy the principle of conservation of energy. There is a difference in method between Maxwell and Boltzmann, inasmuch as Boltzmann measures the probability of a condition by the time during which the system possesses this condition on the average, whereas Maxwell considers innumerable similarly constituted systems with all possible initial conditions. The ratio of the number of systems which are in that condition to the total number of systems determines the probability in question. In conclusion, Maxwell finds, further, that also for any unstable system of very many atoms in rotation under the action of no external forces the mean energy of internal motion is the same for each atom, and that a mixture of gases in a rotating tube behaves exactly as if each gas were present by itself.

Maxwell's proof mentioned above is as follows:—Let there be given any system S obeying the principle of energy. Let its configuration be determined by n generalized coordinates $q_1 \ldots q_n$; let the corresponding momenta be $p_1 \ldots p_n$. (For the sake of clearness I will take occasionally the simplest example, a system of material points acted on by any forces. $q_1 \ldots q_n$ will then denote rectangular coordinates, $p_1 \ldots p_n$ the products of the component velocity into the corresponding masses.)

Let the law of the forces acting in the first system be such that the potential energy V is a given function of the coordinates. Then the motion of the system is completely determined when we know the values $q'_1 \ldots p'_n$ of the coordinates and momenta at the commencement of motion and the time τ which has elapsed. (In the example this means that the coordinates and component velocities at the commencement of motion must be known.) It is then most natural to take the $2n+1$ quantities $q'_1 \ldots p'_n$, τ as so-called independent variables. Since the law of action of the forces is given, all other quantities relating to the motion (*e. g.* the values of the coordinates and momenta after the lapse of the time τ, which Maxwell denotes by

$q_1 \ldots p^n$ without index) may be calculated as functions of these $2n+1$ independent variables. If T be the kinetic energy at the time τ, then $V+T=E$ is the whole energy of the system. These quantities may, of course, also be expressed as functions of the $2n+1$ independent variables*.

If we imagine each of the $2n+1$ quantities $q_1 \ldots p_n E$ actually expressed as a function of the $2n+1$ independent variables, we obtain $2n+1$ equations between $4n+2$ variables. Hamilton's method consists in introducing in place of the independents hitherto chosen, which we may call the "old independents," other independents (the Hamiltonian independents). We may, in fact, from the $2n+1$ equations express any $2n+1$ variables out of the $4n+2$ variables occurring as functions of the remaining $2n+1$. Hamilton supposes the variables $p_1 \ldots p_n, p'_1 \ldots p'_n, \tau$ expressed as functions of $q_1 \ldots q_n$, $q'_1 \ldots q'_n E$; so that the last-named variables play the part of independents. Each of the first-named variables is therefore now to be regarded as a known function of these $2n+1$ independent variables. Starting from these Hamiltonian independents, we easily find

$$\frac{dp'_r}{dq_s} = -\frac{dp_s}{dq'_r}, \quad \frac{dp'_r}{dE} = -\frac{d\tau}{dq'_r}, \quad \frac{d\tau}{dq_r} = \frac{dp_r}{dE}\dagger,$$

where r and s are any equal or unequal numbers.

Just as the product of the differentials of three rectangular coordinates dx, dy, dz may be expressed by the product of the differentials of polar coordinates and then becomes equal to $r^2 \sin\theta \, dr \, d\theta \, d\phi$, so if any m variables $v_1, v_2 \ldots v_m$ are functions of m other $u_1, u_2 \ldots u_m$, the product of the differentials of the first variables may be expressed by the product of the differentials of the latter, by means of the well-known functional determinant

$$dv_1 \, dv_2 \ldots dv_m = du_1 \, du_2 \ldots du_m \, \Sigma \pm \frac{dv_1}{du_1} \frac{dv_2}{du_2} \quad \frac{dv_m}{du_m}. \quad . \quad (1)$$

* E will not contain τ, and will therefore simply be a function of $q'_1 \ldots p'_n$, since it remains constant during the whole motion.

† This follows thus:—If the magnitude $A = 2\int_0^\tau T\,dt$ be expressed as a function of the Hamiltonian independents, then Hamilton shows (Thomson and Tait's 'Natural Philosophy,' new ed. § 330, equation 18) that

$$p'_r = -\frac{dA}{dq'_r}, \quad p_s = \frac{dA}{dq_s}, \quad \tau = \frac{dA}{dE};$$

whence it follows at once that

$$\frac{dp'_r}{dq_s} = -\frac{dp_s}{dq'_r} = \frac{d^2A}{dq_r \, dq'_s},$$

and so on.

We have a special case if some of the v's are identical with some of the u's—if, for example, we retain the z-coordinates and transform only x and y into polar coordinates. Suppose that $v_1=u_1$, $v_2=u_2, \ldots v_k=u_k$, but that $v_{k+1} \ldots v_m$ are given functions of $u_1, u_2, \ldots u_m$; then the functional determinant is simplified to

$$dv_1\, dv_2 \ldots dv_m = du_1\, du_2 \ldots du_m \Sigma \pm \frac{dv_{k+1}}{du_{k+1}} \cdots \frac{dv_m}{du_m}. \quad . \quad (2)$$

We may now apply this general formula to the former one. Instead of $u_1 \ldots u_m$ let us put the $2n+1$ Hamiltonian independents $q_1 \ldots q_n$, $q'_1 \ldots q'_n \mathrm{E}$; for $v_1 \ldots v_k$ let us put $q_1 \ldots q_n$; but for $v_{k+1} \ldots v_m$ let us put $p_1 \ldots p_n \tau$. Then equation (2) becomes

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n\, d\tau =$$
$$dq_1 \ldots dq_n\, dq'_1 \ldots dq'_n\, d\mathrm{E} \Sigma \pm \frac{dp_1}{dq'_1} \cdots \frac{dp_n}{dq'_n} \frac{d\tau}{d\mathrm{E}}. \quad . \quad . \quad (3)$$

Let us now in the generally-applicable formula (2) introduce other special values, viz. for $u_1 \ldots u_m$ the Hamiltonian independents again, but substitute $q'_1 \ldots q'_n$ for $v_1 \ldots v_k$, whilst for $v_{k+1} \ldots v_m$ we substitute the variables $p'_1 \ldots p'_n \tau$; which indeed, according to Hamilton's method, are also functions of the independents introduced by him; consequently equation (2) is applicable to this case just as much as to the former. Equation (2) becomes by this substitution,

$$dq'_1 \ldots dq'_n\, dp'_1 \ldots dp'_n\, d\tau =$$
$$dq'_1 \ldots dq'_n\, dq_1 \ldots dq_n\, d\mathrm{E} \Sigma \pm \frac{dp'_1}{dq_1} \cdots \frac{dp'_n}{dq_n} \frac{d\tau}{d\mathrm{E}}. \quad . \quad . \quad (4)$$

The reader is advised to write down the functional determinants of equations (3) and (4) at length, and then for each member of the functional determinant of equation (4) to substitute the value which it would have according to equation (1). We shall then have, except for sign and for an exchange of horizontal and vertical lines, exactly the functional determinant of equation (3). The two functional determinants have therefore the same numerical value; and since we are here concerned simply with this, and in the equations (3) and (4) the products of the differentials of the right sides are identical, it follows from these equations that

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n d\tau = dq'_1 \ldots d'q_n\, dp'_1 \ldots dp'_n d\tau.$$

Dividing each side by $d\tau$, we obtain

$$dq_1 \ldots dq_n\, dp_1 \ldots dp_n = dq'_1 \ldots dq'_n\, dp'_1 \ldots dp'_n; \quad . \quad (5)$$

which equation expresses Boltzmann's theorem in its fullest generality*.

In this equation the old independents $q'_1 \dots q'_n$, $p'_1 \dots p'_n \tau$ appear again. Since we divided by $d\tau$, and consequently $d\tau$ appears no more in the equation, this is equivalent to saying that the time of the whole motion is to be regarded as a constant. On the other hand, all the coordinates and momenta holding good for the instant of commencement of motion (*i. e.* all the quantities $q'_1 \dots p'_n$) are to be increased by infinitely small amounts. The values $q_1 \dots p_n$ of the coordinates and momenta at the time τ will therefore also undergo infinitely small increase; and, according to equation (5), the product of the first must be put equal to the product of the latter increments; consequently, if we choose the old independents, we must have

$$\Sigma \pm \frac{dq_1}{dq'_1} \cdots \frac{dp_n}{dp'_n} = 1.$$

For the sake of a clear view of the meaning of equation (5), let us imagine, instead of one system S, an infinitely large number of exactly similar systems S. Let the law of action of the forces be precisely the same for all the systems (of course without any two systems having any action upon each other). Let the duration of motion τ be exactly the same for all the systems—but the conditions of the systems at the instant of commencement of motion not the same for all the systems, but having at the instant of commencement of motion the values of coordinates and momenta between the limits q'_1 and $q'_1 + dq'_1 \dots p'_n$ and $p'_n + dp'_n$ for all the systems. Then also at the instant at which the motion ends the conditions of all the systems will not be the same, and coordinates and momenta may lie between the limits q_1 and $q_1 + dq_1 \dots p_n$ and

* In Watson's excellent book, 'A Treatise on the Kinetic Theory of Gases' (Clarendon Press, 1876), p. 13, there is an error, or at least an inaccuracy of expression, in the derivation of this equation. In the partial differential quotients of the functional determinant, at the head of that page, besides p and P, the time τ of the motion is to be regarded as an independent variable; but the equation following from this,

$$\frac{dq_r}{dP_s} = -\frac{d^2A}{dp_r\, dP_s} = -\frac{dQ_s}{dp_r},$$

only holds good when E is variable independently of p and P. Consequently, in forming the partial differential quotients of this equation, E is to be regarded as constant; in forming those of the functional determinant, τ is to be regarded as constant; and the applicability of an equation holding good between the first partial differential quotients to the latter requires still to be proved.

$p_n + dp_n$. Then equation (5) holds good between the products of differentials.

V is a function of the coordinates determined by the constitution of the system; so also T is a function of the momenta or of the momenta and coordinates determined by the constitution of the system. Therefore also $E = V + T$ is a function of coordinates and momenta $F(q_1 \ldots p^n)$ given by the constitution of the system. If we imagine the variables replaced by their values at the time τ, then E appears also as a function of these values, which we have also denoted by $q_1 \ldots q_n$. E may therefore be introduced in the product $dq_1 \ldots dp_n$ of equation (5) in the place of one of the variables, e. g. p_1; so that we obtain

$$dq_1 \ldots dp_n = dq_1 \ldots dq_n\, dp_2 \ldots dp_n\, dE \div \frac{dF(q_1 \ldots p_n)}{dp_1}. \quad (6)$$

This magnitude E, the total energy of the system, is obtained also by substituting in the function F for $q_1 \ldots p_n$ their values $q'_1 \ldots p'_n$ at the commencement of the time. Then E appears expressed as a function of $q'_1 \ldots p'_n$, and may be introduced in the product $dq'_1 \ldots dp'_n$ of equation (5) instead of p'_1, which gives us

$$dq'_1 \ldots dp'_n = dq'_1 \ldots dq'_n\, dp'_2 \ldots dp'_n\, dE \div \frac{dF(q'_1 \ldots p'_n)}{dp'_1}. (6a)$$

If q_1 be expressed as a function of the old independents $q'_1 \ldots q'_n \tau$, then $\frac{dq_1}{d\tau}$ is the differential quotient of q_1 in the usual sense, by allowing the time to increase without altering otherwise the initial conditions $(q' \ldots p'_n)$. Maxwell denotes it by $\dot{q}_1$. It is of course also a function of $q'_1 \ldots p'_n \tau$. Let its value when $\tau = 0$ be $\dot{q}'_1$; then, according to Hamilton,

$$\dot{q}_1 = \frac{dE}{dp_1} = \frac{dF(q_1 \ldots p_n)}{dp_1}, \; \dot{q}'_1 = \frac{dF(q'_1 \ldots p'_n)}{dp'_1}*.$$

Substituting the values (6) and (6*a*) in equation (5) and dividing by dE, we obtain

$$\frac{dq_1 \ldots dq_n\, dp_2 \ldots dp_n}{\dot{q}_1} = \frac{dq'_1 \ldots dq'_n\, dp'_2 \ldots dp'_n}{\dot{q}'_1} \quad \ldots (7)$$

(dE does not occur here). The equation admits of the following interpretation. Let there be given an infinite number of similarly constituted systems S. Let the time of the entire motion have for all exactly the same value τ, and the total energy exactly the same value E. Let the values of the

* Compare Thomson and Tait, new edition, § 318, equation (30).

variables $q_1 \ldots q_n, p_2 \ldots p_n$ at the beginning of the times be between the limits

$$\left.\begin{aligned} q'_1 \text{ and } q'_1+dq'_1 \ldots q'_n \text{ and } q'_n+dq'_n, \\ p'_2 \text{ and } p'_2+dp'_2 \ldots p'_n \text{ and } p'_n+dp'_n, \end{aligned}\right\} \quad . \quad (8)$$

for all the systems, while p_1 is determined by the equation of energy. If, further, we denote the limits between which coordinates and momenta lie at the moment at which motion ends by

$$\left.\begin{aligned} q_1 \text{ and } q_1+dq_1 \ldots q_n \text{ and } q_n+dq_n, \\ p_2 \text{ and } p_2+dp_2 \ldots p_n \text{ and } p_n+dp_n, \end{aligned}\right\} \quad . \quad . \quad . \quad (9)$$

then again equation (7) must hold between the products of differentials.

Maxwell employs now a method which he calls the statistical. He assumes we have a large number N of systems such as S given, having all exactly the same energy E, but whose coordinates and momenta at the commencement of motion have all possible values. He proposes to himself the problem to investigate, not how coordinates and momenta change for each of these systems with the time, but how many systems at a given time "have the phase (pq)"—*i. e.* for how many the coordinates and momenta lie between the limits (9).

p_1 is always determined by the equation of energy. The number of systems which at the time τ "have the phase (pq)" Maxwell denotes in general by

$$\mathrm{N}f(q_1 \ldots q_n, p_2 \ldots p_n\tau)\, dq_1 \ldots dq_n\, dp_2 \ldots dp_n. \quad . \quad (10)$$

The number of systems which, at time 0, have the phase $(p'q')$, *i. e.* for which the variables at this time lie between the limits (8), will consequently be denoted by

$$\mathrm{N}f(q'_1 \ldots q'_n, p'_2 \ldots p'_n 0)\, dq'_1 \ldots dq'_n\, dp'_2 \ldots dp'_n. \quad . \quad (11)$$

But, in accordance with the signification already given to $q_1 \ldots p_n$ and $q'_1 \ldots p'_n$, exactly the same systems have the phase (pq) at the time τ which had the phase $(p'q')$ at the time 0. The expressions (10) and (11) are therefore equal; whence, referring to equation (7), we have

$$\dot{q}_1 f(q_1 \ldots q_n, p_2 \ldots p_n\tau) = \dot{q}'_1 f(q'_1 \ldots q'_n, p'_2 \ldots p_n 0). \quad . \quad (12)$$

Maxwell calls the distribution of the system stationary when the number of systems having any given phase, *e. g.* $(p'q')$, does not change with the time—when, therefore, for any $q'_1 \ldots q'_n, p'_2 \ldots p'_n$,

$$f(q'_1 \ldots q'_n, p'_2 \ldots p_n\tau) = f(q'_1 \ldots q'_n, p'_2 \ldots p'_n 0) \quad . \quad (13)$$

Since in equation (12) $q'_1 \ldots p'_n$ are also any initial values

of the variables whatever, equations (12) and (13) may be at once combined with each other, and give

$$\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n \tau) = \dot{q}'_1 f(q'_1 \dots q'_n, p'_2 \dots p'_n \tau)^*.$$

Since f no longer contains the time τ, it is better to omit τ from under the functional sign and to write

$$\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n) = \dot{q}'_1 f(q'_1 \dots q'_n, p'_2 \dots p'_n). \quad . \quad (14)$$

Here $q'_1 \dots q'_n, p'_2 \dots p'_n$ are any initial values whatever; $q_1 \dots q_n, p_2 \dots p_n$ are the values of coordinates and momenta which a system starting from these initial values attains after a time τ, in other respects unfixed.

Let us therefore imagine a system starting from any initial values of coordinates and momenta; then in course of the motion it will assume continually new and new values of coordinates and momenta. The coordinates and momenta are therefore functions of the initial values and of the time. But there will be in general certain functions of coordinates and momenta which have constant values during the whole motion, as in a free system the component velocities of the centre of gravity, or the sums of angular momenta, are invariable. Let us therefore imagine in the expression $\dot{q}_1 f(q_1 \dots q_n, p_2 \dots p_n)$ first of all those optional initial values from which each system started, then continuously the values in order which coordinates and momenta assume for that system as the time increases; then for the existence of stationary distribution it is necessary and sufficient that the value $\dot{q}_1 f$ shall remain unaltered, or, in other words, $\dot{q}_1 f$ must contain only such functions of $q_1 \dots p_n$ as remain constant during the whole motion of a system from any initial values whatever, and consequently are dependent on the initial values, but not on the time which has elapsed. If the system is so constituted that its coordinates and momenta, starting from given initial values, assume in the course of a sufficiently long time all possible values consistent with the equation of energy, then $\dot{q}_1 f$ must in general have the same value for all coordinates and momenta consistent with the equation of energy—must therefore be a constant.

I will now mention some other terms employed by Maxwell. If one of the systems S starting from a given initial condition moves, all the conditions through which it passes in consequence of its motion as time increases, constitute the

* This or the identical equation (14) is necessary that the distribution may be stationary. It is also sufficient; for from it and equation (12) equation (13) follows at once for any $q'_1 \dots p'_n$ whatever, which is exactly the mathematical expression for a stationary distribution.

path of the system, each separate condition of motion a *phase* of this path. All the functions of coordinates and momenta which remain constant during the whole path he calls the parameter characteristic of the nature of the path, whilst all other functions of coordinates and momenta depend also on the phase. In order that the distribution of the systems shall be a stationary one, it is necessary and sufficient that f shall be equal to $\frac{1}{\dot{q}_1}$, multiplied by an arbitrary function of the parameters characteristic of the nature of the paths.

Maxwell considers the simplest case when this function is a constant, and therefore $f=\frac{C}{\dot{q}_1}$; then

$$\frac{\mathrm{N}C dq_1 \ldots dq_n dp_2 \ldots dp_n}{\dot{q}_1} \quad . \quad . \quad . \quad . \quad (15)$$

is always the number of systems for which coordinates and momenta lie between the limits (9), whilst p_1 is determined by the equation of energy.

This is, then, the simplest possible stationary distribution. If the q's denote the rectangular coordinates of material points, then the products of the component velocities into the corresponding masses m_1u_1, $m_1v_1 \ldots$ are the corresponding momenta; then the kinetic energy

$$\mathrm{T}=\tfrac{1}{2}(m_1u_1^2+m_1v_1^2+\ldots)=\frac{p_1^2}{2m_1}+\frac{p_2^2}{2m_1}+\ldots,$$

where evidently $\frac{m_1u_1^2}{2}$ is the kinetic energy resulting from the motion of the first atom in the direction of the axis of x, and so on. In like manner, generalized coordinates can always be so transformed that

$$\mathrm{T}=\frac{\gamma_1 p_1^2}{2}+\ldots\frac{\gamma_n p_n^2}{2},$$

where the γ's contain simply the coordinates. Maxwell calls $\frac{\gamma_r p_r^2}{2}$ the "kinetic energy resulting from the rth momentum," or simply the "kinetic energy of the rth momentum" The mean kinetic energy of any one of the momenta, say of the rth momentum, is therefore expressed by

$$\frac{\iint\ldots\gamma_r p_r^2 dq_1 \ldots dp_n}{2\dot{q}_1} \div \frac{\iint\ldots dq_1 \ldots dp_n}{\dot{q}_1}.$$

Here the integration is effected with reference to all the other p's before that with reference to p_r.

I will here show only how the integration with reference to p_n is to be effected when r is not equal to n. For q_1 is to be substituted its value

$$\frac{dT}{dp_1}=\gamma_1 p_1=\sqrt{2\gamma_1}\sqrt{E-V-\frac{\gamma_2 p_2^2}{2}\cdots-\frac{\gamma_n p_n^2}{2}}. \quad . \quad (16)$$

If we consider that, in the integration with reference to p_n, the quantities $q_1 \ldots q_n, p_2 \ldots p_{n-1}$, and therefore also $\gamma_1 \ldots \gamma_n$, V, and p_r are to be considered as constant, we may put

$$E-V-\frac{\gamma_2 p_2^2}{2}\cdots-\frac{\gamma_{n-1}p_{n-1}^2}{2}=a, \text{ and } \frac{\gamma_n p_n^2}{2}=x;$$

then, in integrating with reference to p_n, all up to $\dot{q}_1$ comes before the sign of the integral, and $\int\frac{dp_n}{\dot{q}_1}$ reduces to

$$\frac{1}{\sqrt{\gamma_1\gamma_n}}\int_{-\sqrt{a}}^{+\sqrt{a}}\frac{dx}{\sqrt{x}\sqrt{a-x}},$$

which, as is well known, can be easily calculated. The integration can be equally easily performed with reference to the remaining p's, and lastly to p_r. Since V is a given function of the coordinates, the mean kinetic energy may be found simply by repeated integration.

The symmetry of the formula (16) shows at once that it has the same value for all momenta, consequently also for all atoms in the case of material points. The number Z_1 of the systems for which the values of coordinates lie between q_1 and $q_1+dq_1 \ldots q_n$ and q_n+dq_n, and the kinetic energy of the momentum p_r between k and $k+dk$, whilst all the other momenta have all possible values, is found by integrating the expression (15) with reference to those other momenta, but putting $\sqrt{\frac{2k}{\gamma_r}}$ and $\frac{dh}{\sqrt{2k\gamma}}$ for p_r and dp_r; the number Z_2 of the systems for which, whilst keeping the conditions for the coordinates, the last momentum may be any we like, by integrating also with reference to p_r or k over all possible values.

The integration, after using the substitution (16), offers no difficulty, and gives

$$Z_2 = \frac{NC[\Gamma(\frac{1}{2})]^n \sqrt{\gamma_1\gamma_2\ldots\gamma_n}(2E-2V)^{\frac{n-2}{2}} dq_1\ldots dq_n}{\Gamma\left(\frac{n}{2}\right)} *,$$

$$\frac{Z_1}{Z_2} = \frac{(E-V-k)^{\frac{n-3}{2}} dk\,\Gamma\left(\frac{n}{2}\right)}{(E-V)^{\frac{n-2}{2}}\sqrt{k}\,\Gamma(\frac{1}{2})\Gamma\left(\frac{n-1}{2}\right)}.$$

This number, and consequently the law of distribution of kinetic energy, has the same value for all momenta. For large values of n,

$$\frac{Z_1}{Z_2} = \frac{e^{-\frac{k}{2K}} dk}{k\sqrt{2\pi}} \text{ nearly},$$

where $K = \frac{E-V}{n}$ is the mean value of the kinetic energy of a momentum corresponding to these values of coordinates, and the same for all momenta.

In order to apply these equations to the theory of Heat, Maxwell imagines amongst the systems S precisely similarly constituted warm bodies enclosed in absolutely rigid envelopes impermeable to heat, which are completely independent of each other, and all possess the same energy E. The systems S therefore now represent to us very many similarly constituted real bodies of equal temperature and under equal external conditions. The condition of motion of each of these bodies is to be determined by the coordinates and momenta $q_1\ldots p_n$ formerly employed. The different bodies are to have started from very different initial conditions; and the number of systems for which, at the commencement of the time, coordinates and momenta lay between the limits (9), is to be given by the formula (15). We know that then the distribution is a stationary one. The systems which had the phase (pq) at the commencement of the time, it is true, soon pass out of this phase; but exactly as many systems enter on this phase to replace them, and thus it continues for all times. The equations obtained above hold, therefore, for all bodies. The mean kinetic energy must have the same value for each of the momenta, viz. the value calculated above. The case might, of course, occur that the equations should not hold good for each

* When V is small with reference to E, and n is large, $(E-V)^{\frac{n-2}{2}}$ approaches to the limit $E^{\frac{u-2}{2}} e^{-\frac{nV}{2E}}$, and the hydrostatic differential equation for polyatomic gases follows from the equation in the text.

single body—that, for example, the mean kinetic energy of a momentum should be greater in one body than that calculated above, in which case it must of course be smaller again in other bodies, so that we may have the true mean value for all bodies.

But it is to be remembered that all our bodies are found similarly constituted, of equal temperature, and under similar external conditions. In the case just spoken of, therefore, the behaviour of bodies of that kind would be different according to the initial condition from which they started. But this is not confirmed by experience. As often as one and the same body is left to itself with the same energy of motion and under the same external conditions, it assumes with time the same thermal condition, the stationary condition corresponding to that temperature and those external conditions. We are therefore justified in maintaining that our equations hold not simply for the above-defined conceptions of bodies, but also for the stationary final condition of each single warm body. That the condition of equality of temperature between warm bodies has a very simple mechanical meaning independent of their initial conditions, follows also from the fact that it is not influenced by the compression, turning, or displacement of particular parts.

If we substitute for the system S two different gases separated by a solid division-wall permeable to heat, then there follows the equality of the mean kinetic energy of progressive motion of the molecules of both gases, or Avogadro's law; the proof of which, hitherto resting on the equality of this mean kinetic energy in mixtures of gases, is unreliable, since we are not able to show that the mean kinetic energy of progressive motion is the same in mixtures as in separate gases at the same temperature.

The second case discussed by Maxwell is very interesting, but cannot be here reproduced in full. In this $q_1 \dots q_n$ are the rectangular coordinates $x_1 \dots z_n$, therefore $p_1 \dots p_u$ the velocity-components multiplied by the masses $m_1 u_1 \dots m_n w_n$ of a free system of atoms S′ with any internal forces but without external forces. Maxwell introduces into equation (5), instead of $du_1\, dv_1\, dw_1\, du_2\, dv_2 dw_2\, du_3$, the product $d\mathrm{U}\, d\mathrm{V}\, d\mathrm{W}\, d\mathrm{F}\, d\mathrm{G} d\mathrm{H}\, d\mathrm{E}$; where U, V, and W are the velocity-components of the centre of gravity, F, G, and H the constant sums of angular momenta of the elements of motion of the system S′. Equation (5), therefore, after dividing by $\dfrac{d\mathrm{U}\, d\mathrm{V}\, d\mathrm{W}\, d\mathrm{F}\, d\mathrm{G}\, d\mathrm{H}\, d\mathrm{E}}{m_1^3\, m_2^3\, m_3}$, assumes the form

$$\frac{dx'_1 \dots dz'_n\, dv'_3 \dots dw'_n}{a'\, r'\, \dot{r}'} = \frac{dx_1 \dots dz_n\, dv_3 \dots dw_n}{a\, r\, \dot{r}},$$

where r is the distance of the atoms m_1 and m_2, $\dot{r}=\frac{dr}{dt}$; α is the double of the projection of the triangle $m_1 m_2 m_3$ on the yz-plane. If we have again an infinitely great number of similarly constituted systems S′ given for which the magnitudes E, U, V, W, F, G, H have equal values throughout, we find, exactly as before, that the distribution of these systems is stationary when the number of those for which $x_1 \ldots z_n, v_3 \ldots w_n$ lie between the limits x_1 and $x_1+dx_1 \ldots w_n$ and w_n+dw_n, is equal to $\frac{Cdx_1 \ldots dw^n}{\alpha r \dot{r}}$. Maxwell calculates, exactly as in the former case, the number of systems for which, with any velocities, the coordinates of the atoms lie between infinitely close limits, and, further, the number of those for which the velocity-components of an atom also lie between infinitely close limits, the mean kinetic energy of an atom, &c. I quote two only of the results.

1. If ξ, η, ζ be the velocity-components of an atom of mass m referred to new axes of coordinates, of which, at the instant in question, the z-axis passes through the atom, but the two others are axes of the section of the momental ellipsoid by their plane, whose origin at each instant is the centre of gravity of the system, and which revolve with the angular velocities which the system acquired in suddenly becoming solid through the operation of internal forces, then the mean values of the magnitudes $m\zeta^2$, $\frac{m\xi^2}{1-a\gamma z^2}$, $\frac{m\eta^2}{1-b\gamma z^2}$ are the same for all atoms. In particular, the law according to which these magnitudes are distributed amongst the atoms is the same as that according to which mu^2, mv^2, mw^2 were distributed in the former case. At the same time $\gamma=\frac{Mm}{M-m}$; if M is the total mass of the system, z is the distance of the centre of gravity of the system from a straight line passing through the atoms whose direction-cosine, with reference to the new axes of coordinates, are proportional to ξ, η, and ζ. Lastly, $a=\frac{BC-L^2}{D}$, $b=\frac{AC-M^2}{D}$ if A, B, C are the moments of inertia of the system with reference to the new axes of coordinates, L, M, N the sums Σmyz, Σmxz, Σmxy with reference to the system of coordinates, and

$$D=\begin{vmatrix} A, & -N, & -M \\ -N, & B, & -L \\ -M, & -L, & C \end{vmatrix}$$

If the number of atoms is very large, then still $\frac{m(\xi^2+\eta^2+\zeta^2)}{}$,

consequently the mean kinetic energy of internal motion (*i. e.* of that relative to the new axes of coordinates), is the same for the atoms.

2. A gaseous mixture distributes itself in a horizontal tube rotating about a vertical axis, exactly as if each of its constituents were present alone in equilibrium under the action of gravity and of centrifugal force. A tube 1 metre (l) long, with one end in the axis of rotation, must make about ten (n) revolutions per second in order that a mixture of hydrogen and carbon dioxide shall contain at one end 1 per cent. (p) carbon dioxide more than at the other. The rotation must last about two hours in order that the previous deviations from a stationary distribution shall become about one hundred times smaller; p is proportional to the square of the velocity of the moving end of the tube, and therefore to l^2n^2.

XXXV. *Notes on Physiological Optics.* *By* W. Le Conte Stevens*.

In the 'Philosophical Magazine' for May 1882 the present writer discussed certain phenomena of vision under variable physiological conditions. Among these was stereoscopy, attained from a pair of perfectly similar diagrams, with parallelism or slight divergence of visual lines, the binocular resultant image being caused to appear concave, convex, or plane at will, by properly adjusting the cards in position so that the two retinal images from them could be made either slightly dissimilar or alike. A geometric discussion of this was given in connexion with the record of other experiments that illustrated the important effect of muscular action in modifying our unconscious interpretation of retinal sensations. This discussion was preceded by a consideration of the current theory of corresponding retinal points, which was accepted only in a modified sense, and not mathematically. It was assumed that, in examining the binocular resultant, freedom of motion is allowed the eyes—a condition that has usually been found necessary when stereoscopy by this method is performed for the first time by any one who is not skilled in binocular experiments. Even at that date the writer was convinced that play of the eyes was not indispensable, however effective it might be in confirming the visual judgment. The geometric discussion, though correct so far as it extends, was not deemed capable of covering all the facts; but to test the extent to

* An Abstract from two Papers read before the American Association for the Advancement of Science at the Montreal meeting in August 1882. Communicated by the Author.

which it was possible to attain such results without motion of the eyes, it was important to employ the electric spark as a means of illumination. The opportunity of doing so was then wanting, but has since been secured.

Vision by the Light of the Electric Spark.

The apparatus employed for the production of momentary illumination was a large induction-coil belonging to the Physical Laboratory of Columbia College (New York), and loaned for the purpose by Professor O. N. Rood. The stereoscope used was the reflecting instrument described in a former paper*, which had been so constructed as to give for registration the angle, positive or negative, between the observer's visual lines, the distance of each card from the eye that receives its image, and the angle which the plane of this card makes with the visual line, assuming the latter to be horizontal and the axis of rotation of the card to be vertical. The writer was fortunate in securing the cooperation of Mr. W. W. Share, Assistant in Physics in Columbia College, who soon acquired more than usual skill in the control of his eyes for binocular experiments.

In the dark room the stereoscope was first so arranged that parallelism between the two visual lines was necessitated, in obtaining binocular vision of the pair of pictures at the moment these were equally illuminated by the passing of a spark The plane of each card being perpendicularly across the supporting arm of the reflector, the binocular resultant presented the appearance of a series of concentric circles on a flat surface. By rotating each card through a known angle on its vertical axis, the binocular resultant could be made to assume at will the form of a convex or concave elliptic shield. The observer was seated with closed eyes in front of the stereoscope while the manipulator of the apparatus arranged the cards. The observer, not knowing whether this arrangement would produce planeness, convexity, or concavity, was then invited to open his eyes and interpret the binocular retinal sensation attained by the illumination of the cards with a single spark. It was found possible, nearly always, to make a correct interpretation at the first trial. Mr. Share and the writer acted each alternately as observer and manipulator; and the result attained was confirmed by the experience of Professor Rood, who tried the same experiments independently.

The distance and diameter of the circle on each card being known, and also the angle of rotation on its vertical axis, it becomes possible to calculate the maximum retinal displace-

* Philosophical Magazine, December 1881.

ment of images which would have corresponded retinally if the angle of rotation, ϕ, were zero. The attention being specially given to the centre of the binocular concave or convex resultant, the illusion of binocular unity and depth in the picture remained possible when the retinal displacement corresponding to marginal portions of the combined image was as great as ·39 millim., or more than 80 times the diameter corresponding to what has been estimated to be the *minimum visibile*. By giving attention, through indirect vision, to the marginal portions, the illusion of binocular unity was easily destroyed, and double images at once became detectable. The result was confusion and loss of the third dimension in space at these marginal portions, while the perception remained clear for central portions where no duplication could be perceived. These effects were noticed by both Mr. Share and the writer.

The pictures found best in these experiments were concentric circles consisting of broad black bands on a white ground, or of white bands on a black ground. Various other stereographs were employed, many of them constructed for the production of stereoscopic relief, which could be reversed or totally suppressed by appropriate arrangement of the cards on the arms of the stereoscope. The peculiar nature of the relief, whether direct or reversed, was what the observer was requested to ascertain, and with satisfactory results, usually without delay. The most difficult case was that in which one picture consisted of a red diagram on a green ground, the other a green diagram of the same size on a red ground.

A series of experiments, continued through many days, was tried under illumination with the electric spark, by Mr. Share and the writer jointly, to test still further the effect of muscular strain in modifying the unconscious interpretation of the binocular retinal image as discussed in a former paper†. The optic angle was varied from 3° of divergence to 50° of convergence of visual lines, while the stereograph of the moon was again employed, being kept at a fixed distance on the arms of the stereoscope while the observer, under the abnormal conditions imposed, was requested to form an estimate of apparent distance and diameter. Each acted as manipulator and recorder for the other, the observer being kept ignorant of his own record until the whole series of experiments was completed. The result was in each case quite similar to that formerly obtained with vision by continuous light; but the limit of error was much wider, showing that under such unusual conditions no single visual judgment is worthy of any confidence. The general effect on each, however, was that

* Philosophical Magazine, December 1881.

strain of the internal rectus and ciliary muscles produces the illusion that the object perceived is smaller and nearer.

The above is a mere statement of facts. Any discussion they may suggest is reserved for a future paragraph.

The Binocular Union of Spectral Images.

If a sharply defined object be momentarily illuminated by the intense light of the electric spark, a positive after-image is perceived and quickly followed by a negative image of short duration. If the gaze be very steadily fixed upon one point of an object that is strongly illuminated by the direct rays of the sun, the eyes being at the same time protected from the glare proceeding from surrounding objects, a negative after-image is obtained that lasts several minutes. Since its existence is due to fatigue of the retina in certain parts while others remain unfatigued, such an image appears always in the direction of the visual line, changing in apparent position with every motion of the eye.

The late Professor W. B. Rogers, of Boston, published in 1860* some experiments on the binocular union of after-images from illuminated lines so arranged as to produce the appearance of relief. Perspective after-images were likewise obtained by Wheatstone and by Wundt; but an objection to conclusions drawn from such perceptions as these consists in the fact that the observer knows what effects *would* result in direct vision under the conditions imposed; indeed he simply retains a subjective perception of what he has just seen binocularly. It is difficult to determine how far the perception may be due to imagination rather than to immediate retinal sensation. Professor Rogers succeeded in attaining perspective after-images even when the luminous lines were regarded successively instead of together; but thus far no one else seems to have confirmed this result; and the experiment is still liable to the objection that the visual judgment is warped by anticipation and association. Experiments therefore have lately been made with a view to testing these results, and at the same time to ascertain whether any modification would be imposed by varying the muscular conditions under which the spectral images are seen.

1. Across the median plane of vision was held a card with the upper edge more remote than the centre, so that a white band from top to bottom on a dark background was inclined about 40°. This was fixedly regarded with each eye sepa-

* Proceedings of the American Association for the Advancement of Science, 1860, p. 187 *et seq.*

rately in succession, while held in direct sunlight, until both retinas were fatigued. On going then into a slightly darkened room, the inclined spectral image was easily perceived, apparently in mid-air. On making the visual lines parallel, it became projected on the wall, but without losing its obliquity. On strongly contracting the internal rectus muscles, it appeared still directly in front, but much smaller and nearer. The experiment was repeated many times, and varied, but with uniform results.

2. On separate cards a pair of diagrams were constructed in such a manner as to produce an image in relief when binocularly viewed, in the stereoscope or otherwise. These were separately and successively regarded in sunlight, each with the appropriate eye. In the dark room the resultant after-image appeared in mid-air in clear relief. On shutting one eye, the component image that remained visible to the other was at once projected upon the wall as a flat picture. Strongly contracting the ciliary muscle of the eye remaining open, without sensibly contracting the rectus muscles, the picture was made to approach and grow apparently smaller, in almost as marked a degree as by the previous experiment.

3. A series of concentric black and white circular bands was constructed on a card, which was held in a vertical plane obliquely crossing the horizontal visual line of the left eye. After the retina had become fatigued, the same card was held across that of the right eye, but with opposite obliquity, so that the distortions of the elliptic images on the two retinas should be opposite in sense. Each eye was closed while the other was receiving light from the card. The resultant spectral image was concave instead of plane, and presented the same variations with change of muscular conditions as in previous experiments.

4. To ascertain whether these perspective stereoscopic effects were due to imagination and association, or whether they were the immediate outcome of retinal sensation, from the existence of dissimilar images remaining through fatigue in the two eyes, it was necessary to test some one whose eyes were normal, but who was ignorant regarding the nature of the visual effects to be produced, and who therefore could not be influenced by anticipation. It was found possible to enlist the interest of a youth of good general intelligence, who was entirely unacquainted with even the elementary principles of binocular vision. He submitted to be trained until he could secure monocular after-images successfully with either eye at will. Without granting him the slightest clue by which results could be anticipated, the writer employed a pair of cards on

which were diagrams so arranged that the binocular resultant could be made either a raised cone, a flat picture, or a hollow cone, according to the mode of combination selected. These cards were viewed in sunlight, never binocularly, but always separately and in succession, the relation between the pictures being varied in successive experiments. As soon as the retinas were fatigued, the observer was led into a perfectly dark room, and requested to describe the resultant spectral images perceived. Without allowing him ever to know whether his visual judgments were right or wrong, these experiments were repeated day after day, until the youth's own conclusions were definitely formed by repeated interpretation of his retinal sensations. His judgments were in the majority of cases correct, during the latter part of the time invariably so; and by spectral images alone he learned what should be the proper arrangement of pictures to produce a binocular resultant that was concave or convex at will. The cards with concentric circular bands were then substituted; and in like manner he soon learned what kind of obliquity should be given the plane of each card in order to produce a concave or convex spectral binocular image immediately afterwards. His eyes were not sufficiently trained to enable him to test the effect of varying the tension in either ciliary or rectus muscles, nor was he able to perceive duplication in any part of any binocular spectral image.

5. A pair of diagrams were constructed in such manner as to show very plainly the binocular duplication of central parts in the background when the foreground was regarded and the gaze was monocularly directed to the centre of each in succession, with the usual precautions. The spectral image presented the appearance of relief. By an effort of special attention the duplication of the background became perceptible; but at the same moment the appearance of relief was lost.

Results from the Experiments just described.

These experiments, in conjunction with those made by the light of the electric spark, show that in the new mode of stereoscopy play of the eyes is by no means necessary, although it constitutes an important aid in all cases where a clear visual judgment is not attainable at the first glance. They show also very conclusively that the conscious perception of double images in the binocular field of view, on which so much stress was laid by Sir David Brewster*, far from being conducive to clearness of binocular perception, tends rather to interfere

* Brewster, 'The Stereoscope,' p. 76 *et seq.*

with it. If it be said that we unconsciously perceive them and intuitively distinguish between the two kinds, homonymous and heteronymous, this conclusion cannot be confirmed or disproven, except so far as experiments like those just detailed may be accepted as having some bearing upon the subject. The writer's disposition is to discard intuition entirely, and, with Helmholtz*, to regard the degree of attention bestowed upon objects pictured at the same moment on different parts of the two retinas as an element of more importance than either play of the eyes or the perception of double images. The point in the field of view to which most attention is habitually given is that pictured upon corresponding retinal parts; but the attention is at the same moment divided, being given in less degree to many other parts of the field of view as simultaneously perceived with each eye. The mental suggestion due to the impression of non-corresponding parts is that of the third dimension in space. If this be called the perception of double images, their effect seems to be dependent upon their *not* emerging into consciousness. Add to this the fact that the gradation between single and double vision is wholly imperceptible, and hence that for infinitesimal departures from single vision there can be no demonstrable distinction between the two kinds of double images. In the interpretation of our sensations we are certain that experience is our habitual guide, though by no means always a reliable one. Whether intuition can be accepted as an additional guide at all, it is not easy to pronounce. The debate between the advocates of the empiristic and nativistic theories is doubtless like the well-known quarrel about a certain shield, and may be continued indefinitely. The domain of intuition is certainly far more limited than was thought a few generations ago; whether it can be reduced to zero may perhaps be decided a few generations hence. In all ordinary cases of binocular vision the effect is cumulative. The judgment quickly reached is a product not only of difference in the degree of attention given at the same moment to objects seen by direct and by indirect vision respectively, but also of variation in attention to different points directly viewed in succession, of the muscular sense while free play is given to the eyes, and of all the elements available in monocular vision, which have been grouped together under the name of physical in contrast with physiological perspective.

* Helmholtz, *Optique Physiologique*, p. 1009.

XXXVI. *Notices respecting New Books.*

A Treatise on the Transit Instrument as applied to the Determination of Time. For the use of Country Gentlemen. By LATIMER CLARK, *M.I.C.E.&c.* (Published by the author, 6 Westminster Chambers, London.) 72 pages text, with 29 pages Transit Tables.

THE object of this little work is to popularize the use of the portable Transit for finding correct time among amateurs of small means. About half the work, describing the instrument and its use, is written in a thoroughly popular style, as free as is possible from technical terms. Simple modes of adjusting in position are given; and the instrument is supposed to be used only in correct position, so that no "corrections" have to be computed. This is an admirable mode of use for a beginner: some preliminary personal instruction would, however, be required; the detail given is not enough for a person quite unused to instruments. The second part is intended for more advanced amateurs. In this some astronomical terms and usages are explained, and the mode of computing the "corrections" to transits observed with an instrument not in perfect adjustment are fully entered into.

The Tables (72 pages) contain the data for transits of the sun and certain stars for six months in a simple form very suitable for beginners; they form an appendix to the text, and are to be published anew yearly, thus saving the need of mastering the Nautical Almanac (itself rather a formidable work).

There is a misprint of $8° \, 46' \, 28''$ for $8^h \, 46^m \, 28^s$ on p. 42, which may confuse a beginner. The accounts of the Polestar on p. 26 and p. 48 do not agree: on p. 26 it is said that it "is very close to the North Pole, and revolves round it daily at a distance of about $1\frac{1}{2}$ degree;" whilst on p. 48 it is said, "let us imagine ourselves at the North Pole. We should see the Polestar directly overhead remaining motionless." On the whole, however, the work may be said to fulfil well the purpose for which it was written, and will help to supply an amateur's wants. To meet the case of amateurs of small means, it is stated (in an advertisement) that an excellent portable transit with $14''$ telescope and $1\frac{1}{2}''$ aperture can now be had for £8.

ALLAN CUNNINGHAM, *Major R.E.*

Geology of Wisconsin. Survey of 1873–79. Vol. III. Large 8vo, 763 pages. With numerous Plates and other Illustrations, and an Atlas of Maps. Published under the Direction of the Chief Geologist [T. C. Chamberlin] by the Commissioners of Public Printing, in accordance with Legislative Enactment. [Madison.] 1880.

VOLUME II. of this excellent Survey, published in 1877, and noticed in this Journal for April 1880, p. 302, treated of the geology of the eastern, central, and south-western portions of the State of Wisconsin. In the volume before us the extreme north-western and north-eastern portions of the State are described. The

former area is bordered by Lake Superior, between Minnesota and Michigan; the latter, with a part of the iron district of Michigan annexed, is traversed by the Menoninee River and some of its affluents.

Part I. of this volume consists of valuable observations by Professor Roland D. Irving on the "General Geology of the Lake-Superior Region," which is estimated at 70,000 square miles in area, with strata, probably more than 100,000 feet in thickness, including four great unconformable systems; and the whole is coated with enormous deposits of glacial drift, besides lake-alluviums. I. The Laurentian gneiss forms the crystalline nucleus of the region, and is continuous with that of Canada. With some associated and often gneissoid granite, these altered strata are greatly folded, and have a general southerly dip, with an E.–W. strike, and an enormous thickness. II. The Huronian rocks are 12,800 feet thick, and consist of (from below upwards):—1. crystalline tremolitic limestone and a partial quartzite, 130 feet; 2. quartz-schist, mica-schist, and some novaculite, 410 feet; 3. tremolitic magnetite-schists and irony quartzites (Penokee Iron Range), 780 feet; 4. black mica-slates, with diorite and schistose quartzites, &c., 3495 feet; 5. dark-grey mica-schists, with intrusive granite, 7985 feet. The dip is northward, and the strike is oblique to that of underlying Laurentians. III. The Keweenawan or Copper-bearing series succeeds, seven miles in thickness, and consists of distinctly stratified igneous rocks like great flows and ash-beds. The lower rocks, from 10,000 to 30,000 feet, are almost wholly augite-plagioclase—namely, diabase, melaphyr, and gabbro (Rosenbusch). Shales, sandstones, and conglomerates then come in, and, becoming more and more frequent, nearly exclude the igneous rocks for the uppermost 15,000 feet of the series. IV. Lying on the eroded surface of the Keweenawan strata is a great horizontal set of sandstones, defined as the "Lake-Superior Sandstone," and regarded as equivalent to the Potsdam Sandstone of the Mississippi valley.

In Part II. Professor Raphael Pumpelly gives the "Lithology of the Keweenawan System," specimens of which were sent to him, and represented Diabase, Melaphyr, Gabbro, Uralitic Gabbro, Uralitic Diabase, Augite-diorite, and Felsitic Porphyries.

Part III., by Prof. R. D. Irving, describes the "Geology of the Eastern Lake-Superior District" of Wisconsin, premising its Topography, with Altitudes, Drainage-system, Vegetation, and Soils. The lithology, stratification, and economics of each of the great systems are given in detail according to locality; also an account of the Glacial Drift and Lacustrine Clays (Champlain Series).

Part IV., by Mr. C. E. Wright, treats of the "Huronian Series West of Penokee Gap." The Penokee Iron Range is here especially described, with the details of method of examination. The magnetic bands being covered by Drift, their breadth and extent were defined by the use of the solar dial-compass and the dipping needle, both of which are succinctly described. The iron-ores are, it seems, nearly all poor or "lean," the good ore being probably high up in the series.

Part V., by Mr. E. T. Sweet, gives the topography, natural-history, geology, lithology, and economics of the "Western Lake-Superior District" of the State. Among the Quaternary deposits occur the Moraines and Pot-holes of the Kettle Range, similar to that of Eastern Wisconsin, described in the previous volume. In the Glacial Drift occur not unfrequently nuggets and boulders of native copper. Some of the granitic and gneissic boulders must have crossed Lake Superior and travelled at least 200 miles.

Part VI. consists of an account of the "Geology of the Upper Saint-Croix District," based on the Notes of the late Mr. Moses Strong, edited by Mr. C. T. Chamberlin. It treats of the Surface-features, the Quaternary formations, and the Older formations, both generally and in detail. This area is inland and south of the "Western Lake-Superior District."

The Menominee Region, including parts of both Wisconsin and Michigan, is described geologically and lithologically in Parts VII. and VIII. by Major Thomas Benton Brooks and Mr. E. T. Street. This being an important iron-district, overrunning the boundary of the two contiguous States, and Wisconsin not supplying any money for the Survey beyond its own border, Major Brooks completed the work at his own expense, and suffered serious illness also from his labours. Besides the Superficial Deposits of Drift &c., the country has:—1. The Calciferous sand-rock and limestone and the Saint-Mary's (Potsdam) sandstone, of the Lower Silurian; 2. None of the Copper-series; 3. The Upper-Huronian granite, gneiss, schists (hornblende, actinolite, mica, chlorite, and quartz), iron-ores, clay-slate, carbonaceous slate or graphitic shale, quartzite, and conglomerate. 4. Middle-Huronian clay-slate and quartzite. 5. Lower-Huronian dolomite, iron-ore, and quartzite. 6. Laurentian granite, gneiss, and crystalline schists. Three elaborate Tables of the rocks and their component minerals in the Menominee and Marquette Regions present a summary of the lithological characters of the several systems and series of rocks and of their relative abundance and stratigraphical order. The descriptive lithology of the Menominee rocks and of the Huronian rocks south of Lake Superior form two interesting chapters (Chapters 3 and 4) of Part VII.; and are followed by Dr. Arthur Wichmann's microscopical investigations in the Huronian rocks, prefaced with a technical account of the minerals composing the said rocks. Besides this eminent lithologist of Leipsic, others have aided in the microscopical lithology of Wisconsin as treated in this volume, namely E. Törnebohm, F. Zirkel, Herr Wapler, S. Allport, Frank Rutley, G. J. Brush, J. D. Dana, G. W. Hawes, A. A. Julien, T. Sterry Hunt, Prof. R. Pumpelly, T. B. Brooks, and C. E. Wright. Nine coloured plates of microscopic sections of rocks occur in the volume, and enhance the value of the lithological descriptions. The lithographic and chromolithographic views, maps, and sections, illustrating the topography, geology, and mining, are numerous (44); there are also 23 woodcuts, chiefly sections of strata. Above all, the magnficent Atlas of Plates XVII. to XXX. inclusive, giving

many sections besides the coloured geological maps, is to be noticed as a most useful adjunct to this liberally published Report on the geological structure and capabilities of important parts of the great State of Wisconsin.

The Life of Immanuel Kant. *By* J. H. W. Stuckenberg, *D.D., late Professor in Wittenberg College, Ohio.* London: Macmillan and Co. 1882.

The work before us does not answer completely to its title. Our notion of a *Life* involves in its essence the being written chronologically. To put every thing in its proper place with respect to time may be difficult in the case of a philosopher who flourished one hundred years ago and whose life was remarkably uniform; but the task so accomplished would be more interesting to the general reader and more valuable to a philosopher. For example, at the end of Chap. XI. we are landed in the "return" to Kant's philosophy, while in Chap. XIII. we are led back to consider the old age and death of the philosopher himself. Our author is in consequence apt to fall into redundancy, a danger of which he is himself conscious; for he says at the beginning of Chap. XII., devoted to Correspondence and Correspondents, "Kant's letters have already been so extensively used in this biography, that little more need be said of them." In all other respects the workmanship of the volume seems to us most praiseworthy.

Our author in his Preface says, "If Kant's works throw light on his life, it will also be found that his life aids materially in understanding his works." In the thirteen following chapters he aims at giving all the data which can by any possibility throw light on the views of the philosopher—scientific, moral, and religious. There are data given, however, which one would think cannot throw much light; for example, an account given of his method of retiring (p. 435), the nature of which may be inferred from the following specimen:—"In summer one, in winter two nightcaps were worn."

With this book before him, the scientific man will be able to appreciate the qualifications, natural and acquired, which Kant brought to the task undertaken in the 'Critique of Pure Reason.'

XXXVII. *Intelligence and Miscellaneous Articles.*

CONSERVATION OF SOLAR ENERGY.

BY PLINY EARLE CHASE, LL.D.*

All forms of solar energy are due to solar radiation. The maintenance of the energy depends on the maintenance of the radiations. In investigating the relations of centripetal and centrifugal action and reaction, it seems desirable to consider the following hypotheses and conclusions:—

* Abstract of a Paper read before the American Association at Montreal, August 25, 1882. Communicated by the Author.

1. Laplace's estimate that the velocity of transmission, in gravitating acceleration, if finite, must be at least 100,000,000 times as great as the velocity of light.

2. Le Sage's hypothesis that gravitation and luminous radiation represent equal actions and reactions.

3. Faraday's search for a gravitating constant.

4. Herschel's comparison of the mean *vis viva* of light with that of sound.

5. Weber's identification of the velocity of light (v_λ) with the "electromagnetic ratio" (v_ϵ).

6. Berthelot's "explosive waves," and their action upon sound-waves.

7. The inquiries of Siemens into the combined influence of rotation, centrifugal action, gravitating fall, and chemical affinity.

To these considerations the following may be added:—

8. If there is a natural unit of force, we may look for a natural unit of velocity.

9. Oscillations may be orbital, pendulous, or wave.

10. Different transformations of similar oscillations are harmonic.

11. Rotation may be regarded as a pendulous motion, due to retarded and modified revolution.

12. The resemblance of Le Sage's theory to the kinetic theory of gases points to a probability that the natural unit of velocity is oscillatory. This probability is strengthened if we assume the existence of molecular and intermolecular elasticity.

13. In looking to the activities of the principal mass in our system for indications of a natural unit of velocity, we find that gravitating velocities may be represented by gt.

14. In order that gt may be constant, t must vary inversely as g, and therefore directly as r^2. This variation is found in the rotation of a nebulous sphere, where it holds good for all stages of expansion or contraction which are not affected by external influence.

15. Gravitating acceleration should do its whole work in stellar rotation as well as in planetary revolution.

16. Particles exposed to solar superficial gravitating acceleration, during a single oscillation of half-rotation, would acquire a velocity which is equivalent to the velocity of light. If we designate this acquired velocity by v_γ, we have $v_\gamma=gt=v_\lambda$ as a gravitating constant, which gives the following extension to Weber's analogy: $v_\lambda=v_\epsilon=v_\gamma$. In other words, the unit of velocity which is indicated by solar gravitation is the same as is indicated by light and by electricity.

17. The velocity of light, like the velocity of sound, thus represents an elastic atmosphere whose height, if homogeneous, would be twice the virtual fall which would give the velocity in question, and whose elasticity is in harmonic accordance with solar rotation and planetary revolution.

18. Subsidence, from Laplace's limit of synchronous rotation and revolution to the poles, gives a mechanical equivalent of 76,000,000 J for each pound of subsiding matter. The spiral character of the

subsidence produces solenoidal currents, which may help to explain the equality of v_λ, v_ϵ, and v_γ.

ON THE APPEARANCES OF THE ELECTRIC ARC IN THE VAPOUR OF BISULPHIDE OF CARBON. BY M. JAMIN, WITH THE ASSISTANCE OF M. G. MANEUVRIER.

At the meeting of the 19th June I made known to the Academy the modifications undergone by the electric arc in the vacuum of an air-pump when the arc is produced by a Gramme machine with alternating currents of high tension. I soon perceived that the appearances are modified if gases or vapours are introduced into the glass vessel in which the experiment is made. In the vapour of bisulphide of carbon they are very remarkable.

The burner is formed by two parallel vertical carbons fixed at their bases; the upper extremities, which face each other, can by a simple mechanism be joined or separated. The apparatus is placed under a large receiver of an air-pump, in which a vacuum as complete as possible is produced. It is known that then the arc is not formed; it is replaced by the gleams of a Geissler tube; but when a few drops of bisulphide of carbon are introduced, so as to obtain an increase of pressure of 5 or 6 centim., the arc is seen to kindle between the points when they touch, and to persist after they are separated.

At that moment there is as it were an explosion of light, so vivid as to be insupportable, incomparably superior to the usual brightness of the arc. On looking at it through a dark-coloured glass, one sees a brilliant arc 5 or 6 centim. in height, resembling a horseshoe or a capital omega. The two extremities are at the two carbon points. Besides this a long flame is seen like that of a hearth, which overhangs the arc, escapes from it, and ascends vertically.

The points of the carbons appear red and very brilliant; but the arc is pale green; and as its light dominates that of the carbons, the whole room is illuminated with that tint, as it would be by a Bengal light with copper. The brightness increases with the increase of tension of the vapour, until it becomes intolerable; but as the resistance of the medium is augmented at the same time, the arc often goes out, and it is necessary to relight it every moment by joining the two carbons.

Examined with the spectroscope this light presents all the lines of carburetted gases in combustion, but more complete and sharper. They are those described by M. Thollon at the meeting on August 1, 1881. The spectrum is very discontinuous. At its red end a grooved region was seen—first a very bright line followed by several others thinner and close, then a broader line a repetition of the first and likewise followed by fine lines; these appearances were repeated in going towards the orange, but growing weaker till they disappeared. After a dark interval the same appearances were seen again in the yellow and the beginning of the green; then there

was a dark interval, then the repetition of the same effects in the green, and finally in the violet.

In brief, the spectrum is composed of four grooved portions, in the red, yellow, green, and violet, so identical that they might be taken, except the colouring, for one and the same design which had travelled from the red to the violet. It is quite probable that they obey one and the same harmonic law, which remains to be discovered.

Of these four regions the green is the most luminous; it is that which gives the special tint taken by the arc and colouring all objects green.

During the manifestation of these appearances a chemical action takes place. If any air has been left in the receiver, or if the apparatus is not quite closed, the bisulphide of carbon undergoes incomplete combustion, a mist of sulphur fills the space and is deposited on the sides; the carbon burns alone. If the air has been well purified, the mist does not form; a brown deposit settles on the sides, becomes black, sticks to the glass, and tarnishes it. This deposit is volatile; its odour reminds one of that of sulphur.

It is evidently a compound of sulphur and carbon, perhaps a protosulphide corresponding to the oxide of carbon, perhaps an isomeric modification of the ordinary sulphide. In fact, neither a deposit of sulphur nor one of carbon is seen, and the carbons of the burner have neither lost nor gained any thing. It is probable that the bisulphide of carbon is dissociated, the sulphur volatilized, the carbon in vapour disseminated in the arc, and that this carbon and this sulphur recombine in the flame to reconstitute a combination under different conditions. But this is only a conjecture, no analysis having yet been made.

To recapitulate, this experiment is remarkable for the extraordinary quantity of light produced, the magnitude of the arc, its colour, the composition of its spectrum, and the chemical actions which take place. It is not likely that it can ever be turned to account for lighting, on account of its colour, unless perhaps for light-houses or distant signals.—*Comptes Rendus de l'Académie des Sciences*, July 3, 1882, t. xcv. pp. 6, 7.

ON THE ELECTRIC RESISTANCE OF GLASS AT LOW TEMPERATURES. BY G. FOUSSEREAU.

The method employed consists in passing the electricity supplied by a Volta's pile of from 1 to 100 elements across a reaction-tube of 1–2 centim. diameter and very regular thickness, closed at one end. The electricity is collected in a condenser of known capacity, the two armatures of which are connected with the two mercuries of a Lippmann electrometer of measured capacity. The time necessary for communicating to the mercurial column of the electrometer a displacement corresponding to a determined difference of potential is observed.

The reaction-tube dips into a wider test-tube; and its two faces

are bathed, up to a known height, by two conducting masses of concentrated sulphuric acid, into which dip some platinum wires carefully insulated from the sides above the level of the liquid. This apparatus is surrounded by a glass "muff," the air of which is dried by sulphuric acid before commencing the experiments.

In order to obtain a uniform and slowly variable temperature, the base of the apparatus is inserted, up to a level considerably above that of the acid, in an oil-bath, which is itself surrounded by a sand-bath which can be heated progressively. For the sand-bath a refrigerating mixture can be substituted. The observations were extended to $-17°$ C.

If E designates the electromotive force of the pile, ρ_1 and ρ_2 the internal and external radii of the tube, h the height of the liquid, r the specific resistance of glass per cubic centimetre, C the sum of the capacities of the condenser and electrometer, and e the difference of potential communicated to the electrometer (always very small in proportion to E), we have, expressing that the quantity of electricity transmitted through the glass in the time θ has been employed in charging the condenser,

$$r = \frac{2\pi h \mathrm{E}}{\mathrm{C}e \log n \frac{\rho_2}{\rho_1}} \times \theta.$$

Several experiments, made with different heights of sulphuric acid, permit the elimination of the influence of the bottom of the tube, the thickness of which is not the same as that of the sides.

At the instant of the completion of the circuit the glass tube is at first charged like a condenser. Its interior layers afterwards gradually absorb a certain charge of electricity, necessary for bringing them into the definitive state corresponding to the fall of potential established between the surfaces. During this variable state, more or less prolonged according to the nature of the glass, the effects of the charge of the glass are superposed to those of the conductivity. The observations are commenced when the time occupied in charging the glass has assumed a constant value.

I have also observed that rapid heating determines an apparent increase of conductivity greater than the normal increase; in like manner a rapid lowering of the temperature gives rise to an exaggerated resistance: but these phenomena quickly disappear, to give place to the normal resistance; and they are not again produced when the variations of temperature are slow *.

My observations have hitherto been made on three kinds of glass —common glass (with a base of soda and lime), Bohemian glass, and crystal.

In all three, raising the temperature produces a rapid increase of conductivity; the resistance can be expressed by exponential func-

* These phenomena appear to be due to variations in the dielectric power of glass under the influence of temperature.

tions of the form

$$\log x = a - bt + ct^2.$$

(1) For common glass, of density 2·539, expressing the resistances per cubic centim. in millions of megohms, we get the following results:—

Temperatures.	Resistances.
+61°·2	0·705
+20°	91·0
−17°	7970·0

In order to form an idea of the magnitude of this last resistance, it may be remarked that it represents nearly twice the resistance of a copper wire, of 1 square millim. section, reaching from the earth to Sirius.

The whole of the results obtained upon common glass are expressed by the formula

$$\log x = 3{\cdot}00507 - 0{\cdot}052664 \times t + 0{\cdot}00000373 \times t^2.$$

The term of the second order being very small, the values of $\log x$ are represented by a line which differs but little from a straight line. The resistance varies nearly $\frac{1}{9}$ of its value for each degree of temperature.

(2) Bohemian glass of density 2·431, upon which I worked, has from 10 to 15 times the conductivity of common glass at the same temperatures. Its resistance is given by the formula

$$\log x = 1{\cdot}78300 - 0{\cdot}049530 \times t + 0{\cdot}0000711 \times t^2.$$

(3) The crystal tried has for its density 2·933; and it, contrary to Bohemian glass, has from 1000 to 1500 times the insulating-power of ordinary glass at the same temperatures. Its conductivity only begins to be manifest at above 40°.

At 40°·2 its resistance is equal to 6182
At 105° „ „ ...... 11·6

The results are represented by the formula

$$\log x = 7{\cdot}22370 - 0{\cdot}088014 \times t + 0{\cdot}00028072 \times t^2 *.$$

—*Comptes Rendus de l'Académie des Sciences*, July 31, 1882, t. xcv. pp. 216–818.

ON THE SURFACE-TENSION OF SOME LIQUIDS IN CONTACT WITH CARBONIC ACID†. NOTE BY S. WROBLEWSKI‡.

If instead of water we take a liquid which mixes in all proportions with liquid carbonic acid—for instance, alcohol, essential oil

* The experiments were made in M. Jamin's laboratory at the Sorbonne.

† Abstract by the Author.

‡ See the preceding Note, Phil. Mag. Sept. 1882, p. 236.

of turpentine, ether, chloroform—the phenomena assume the following form:—

The surface-tension also diminishes with the increase of the pressure under which the gas is placed; the velocity of the diminution is also much greater at a low than at a higher temperature; but the surface-tension, instead of stopping at a minimum which would be something characteristic of the liquid, falls rapidly; and at 0° C., under the pressure at which carbonic acid is liquefied, all the liquids above mentioned, without distinction, have the surface-tension of carbonic acid—that is, a tension equal to $4\cdot6 \frac{\text{dyne}}{\text{centimetre}}$.

But can they in this state be regarded as the same liquids? Not at all. Let us take water as an example. When it is saturated with carbonic acid under the pressure of one atmosphere only, has it the properties of pure water? It has a different density, a different coefficient of expansion by heat; even the temperature of its maximum density is changed. The changes which take place in the liquids mentioned are much more considerable: we need only observe what takes place with ether when it absorbs carbonic acid under pressure. Its volume increases with such rapidity that, although my method enables me to measure the surface-tension of a liquid in a much shorter time than a minute, it is almost impossible to take exact measurements in this case.

It follows from all these facts that the phenomena described in these Notes have absolutely nothing to do with pressure. The diminution of the surface-tension of the liquids depends solely on the circumstance that the surface-tension of carbonic acid, with which they are compressed, is extremely slight. While the surface-tension of water is equal to $80 \frac{\text{dyne}}{\text{centimetre}}$ nearly, and those of the other liquids examined, with the exception of ether, lie between 32 and $25 \frac{\text{dyne}}{\text{centimetre}}$, the tension of carbonic acid is only $4\cdot6 \frac{\text{dyne}}{\text{centimetre}}$.

Capillary phenomena depend only on the molecular forces acting at the surface of the liquid. Therefore it is not surprising that the presence of a body which possesses so feeble a tension among the particles of the liquid of which that surface-layer is composed exerts an influence upon the resultant of all those forces—that is to say, upon the surface-tension.

The same mode of reasoning applies to the case of a liquid being compressed with a much less liquefiable gas than carbonic acid—such as oxygen, nitrogen, or hydrogen; for the more difficult it is to liquefy those gases the less must be their surface-tension. The effect of the compression will depend only on the value of that tension and on that of the solubility of the gas.—*Comptes Rendus de l'Académie des Sciences*, August 14, 1882, t. xcv. pp. 342, 343.

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XXXVIII. *Comparison of Methods for the Determination of Resistances in Absolute Measure.* By LORD RAYLEIGH, *F.R.S., Professor of Experimental Physics in the University of Cambridge*.*

AT the present time, and in view of the projected conference at Paris, the subject of the present paper is engaging a large share of attention; and Prof. G. Wiedemann† has published an interesting discussion of some of the methods that have been employed. I have thought it might be of service if I also were to place upon record the views that I have been led to entertain, and which are the result of a good deal of experience.

Resistance being of the dimensions of velocity, its absolute measurement involves the absolute measurement of a length and of a time. The latter is usually the time of a vibration of a suspended magnet, and can be determined without much difficulty. In the B.A. method it is the time of rotation of the revolving coil, and can be obtained with all desirable accuracy. In this respect there is not much to choose between one method and another; but when we come to consider the manner in which the linear measurement enters, important differences reveal themselves. These will be discussed in detail presently; but for the moment it will be sufficient to say that

* Communicated by the Author.

† "Ueber die bisherigen Methoden zur Feststellung des Ohm." Separatabdruck aus der *Electrotechnischen Zeitschrift*, July 1882. [Phil. Mag. for October, p. 258.]

the presumption is in favour of any method which requires only a single linear measurement. It is true that this question cannot be decided without regard to the subject of the measurement; but, with scarcely an exception, it is necessary to know the mean radius of a *coil* of several layers of insulated wire. This is apparently the measurement which fixes the limit of final accuracy; and, in comparison with it, determinations of the distances of mirrors and scales &c. are of secondary difficulty.

It will be convenient now to enumerate the principal methods which have been proposed for determining absolute resistances. Minor details, which are not likely to influence the final value of the results, will in general be passed over.

I. Kirchhoff's *Method*, Maxwell's *Electricity and Magnetism*, § 759.

The magnitude of a continuous battery-current in a primary coil is compared with that of the transient current induced in a secondary coil when the primary circuit is removed. Rowland* effected an important improvement by simply reversing the battery-current without motion of the primary coil. The time of vibration of the ballistic galvanometer employed for the transient current is the principal time-measurement. In Rowland's investigation a second galvanometer was employed for the battery-current, and the ratio of constants had to be found by auxiliary experiments. In Glazebrook's† recent determination by this method only one galvanometer was used, the battery-current being reduced in a known manner by shunting. It is shown that the evaluation of the resistance-ratios presents no serious difficulty.

Let h denote the ratio in which the primary current is reduced when it produces a deflection α upon the galvanometer, θ the throw from rest due to the induction-current when the battery is reversed, T the time of vibration of the needle measured from rest to rest, M the coefficient of induction; then the resistance of the secondary circuit in absolute measure is given by

$$R=\frac{\pi \tan \alpha M}{T \,.\, \sin \frac{1}{2}\theta} \div h.$$

Whenever, as in this method, the conductor whose resistance in absolute measure is first determined is composed of copper, frequent comparisons are necessary with standards of German silver or platinum-silver. Otherwise a variation of temperature of about $\frac{1}{4}$ of a degree Cent., which can hardly be detected

* American Journal, xv. 1878.

† Proc. Roy. Soc. June 1882.

with certainty by thermometers, would influence the result by as much as one part in a thousand.

If it be granted that the comparison of currents and the reference to the standard of resistance can be effected satisfactorily, we have only to consider the amount of error involved in the determination of M, the coefficient of mutual induction between the two circuits, which is the fundamental linear measurement. If the two coils are of very nearly the same size, it appears from symmetry that the result is practically a function of the mean of the mean radii only, and not of the two mean radii separately. It is also of course a function of the distance between the mean planes b. Leaving out of consideration the small corrections necessary for the finite size of the sections, we consider M as equal to $4\pi\sqrt{Aa}$ multiplied by the function of γ, given in tables appended to the second edition of Maxwell's 'Electricity,' where

$$\sin\gamma = \frac{2\sqrt{Aa}}{\sqrt{\{(A+a)^2+b^2\}}},$$

or, if we identify A and a with their mean (A_0),

$$\tan\gamma = \frac{2A_0}{b}.$$

The error in M will depend upon the errors committed in the estimates of A_0 and b. If we write

$$\frac{dM}{M} = \lambda\frac{dA_0}{A_0} + \mu\frac{db}{b},$$

then, since M is linear,

$$\lambda+\mu = +1.$$

Thus, if b were great relatively to A_0,

$$\lambda=4, \quad \mu=-3,$$

a very unfavourable arrangement, even if it did not involve a great loss of sensitiveness. The object must be so to arrange matters that the errors in A_0 and b do not multiply themselves unnecessarily in M. But since μ is always negative, λ must inevitably be greater than unity.

The other extreme case, in which b is very small relatively to A_0, may also be considered independently of the general tables; for we may then take approximately (Maxwell's 'Electricity,' § 705)

$$M = 4\pi A_0 \log\left\{\frac{8A_0}{b} - 2\right\},$$

whence

$$\mu = -\frac{1}{\log(8A_0/B) - 2},$$

showing that as b diminishes μ approaches zero, and accordingly λ approaches unity, as is indeed otherwise evident. But when b is small, it is the absolute error db which we must regard as given rather than the relative error db/b; and thus we are directed to stop at a moderate value of b, even if the increased correction necessary for the size of the section were not an argument in the same direction.

The following intermediate cases, calculated by the tables, will give an idea of the actual conditions suitable for a determination by this method:—

γ.	$b/2A_0$.	λ.	μ.	M.
60°	·577	2·61	−1·61	·316
70	·364	2·18	−1·18	·597
75	·268	1·98	− ·98	·829
80	·176	1·76	− ·76	1·186

We may say that the error in the distance of mean planes will reproduce itself something like proportionally in the final result, and that the error of mean radius will be doubled.

Any uncertainty in the actual position of the mean planes relatively to the rings on which the wire is wound may be eliminated, as Glazebrook has shown, by reversing the rings relatively to the distance-pieces.

This method is subject to whatever uncertainty attaches to the use of a ballistic galvanometer*. In its favour it may be said that the apparatus and adjustments are simple, and that no measurements of distances between mirrors and scales is necessary for the principal elements. It should be noticed also that the error due to faulty determination of the distance of mean planes can be eliminated in great measure by varying this quantity, which can be done over a considerable range without much difficulty or expense.

With reference to the capabilities of the method for giving results of the highest accuracy when carried out in the most ambitious manner, it is important to consider the effect of increasing the size of the coils. The coils used by Glazebrook have a mean radius of about 26 centim.; the axial and radial breadths of the section are each about 2 centim. If we suppose the mean radius and the sides of the section to be doubled, the number of turns (about 800) remaining unaltered, the sensitiveness would be increased both by the doubling of M and by the diminished resistances of the coils, while at the

* See Phil. Trans. 1882, p. 669.

same time the subjects of the linear measurements would be of more favourable magnitudes. To enhance the latter advantage, it would probably be an improvement to diminish the radial breadth of the section, on which much of the uncertainty of mean radius depends. In either case it is clear that the limit of accuracy obtainable by this method has not yet been reached.

II. Weber's *Method by Transient Currents,* Maxwell § 760.

"A coil of considerable size is mounted on an axle so as to be capable of revolving about a vertical diameter. The wire of this coil is connected with that of a tangent-galvanometer so as to form a single circuit. Let the resistance of this circuit be R. Let the large coil be placed with its positive face perpendicular to the magnetic meridian, and let it be quickly turned round half a revolution. There will be an induced current due to the earth's magnetic force; and the total quantity of electricity in this current in electro-magnetic measure will be

$$Q=\frac{2g_1H}{R},$$

where g_1 is the magnetic moment of the coil for unit current, which in the case of a large coil may be determined directly by measuring the dimensions of the coil and calculating the sum of the areas of its windings; H is the horizontal component of terrestrial magnetism; and R is the resistance of the circuit formed by the coil and galvanometer together. This current sets the magnet of the galvanometer in motion."

"If the magnet is originally at rest, and if the motion of the coil occupies but a small fraction of the time of a vibration of the magnet, then, if we neglect the resistance to the motion of the magnet, we have, by § 748,

$$Q=\frac{H}{G}\frac{T}{\pi}2\sin\tfrac{1}{2}\theta,$$

where G is the constant of the galvanometer, T is the time of vibration of the magnet, and θ is the observed elongation. From these equations we obtain

$$R=\pi G g\frac{1}{T\sin\frac{1}{2}\theta}.$$

The value of H does not appear in this result, provided it is the same at the position of the coil and at that of the galvanometer. This should not be assumed to be the case, but should be tested by comparing the time of vibration of the

same magnet, first at one of these places, and then at the other."

If a be the mean radius of the coil of the inductor and A that of the galvanometer, we may write, neglecting the corrections for the finite sizes of the sections,

$$g=\pi a^2, \quad G=\frac{2\pi}{A};$$

so that

$$gG=2\pi^2\frac{a^2}{A}.$$

This is the linear quantity of the method. With respect to the chances of error in determining it, we see that the error of the mean radius of the inductor enters doubly, and that of the mean radius of the galvanometer enters singly. Probably in this respect there is not much to choose between this method and the use in method I. of the same coils placed at a moderate distance apart.

A colossal apparatus for the use of the present method has been constructed and tested by MM. W. Weber and F. Zöllner*, the coils of which are as much as 1 metre in diameter. The principal difficulty arises in connexion with the galvanometer-magnet. Two magnets were used whose lengths were respectively 200 millim. and 100 millim.; and the results obtained in the two cases differed by as much as 2 per cent. The discrepancy is doubtless due to the influence of the finite length of the magnets causing the magnetic poles to be sensibly distant from the centre of the coil, for which point the effects are calculated; and the disturbance will be proportional to the square of the distance between the poles, or more properly to the "radius of gyration" of the ideal magnetic matter about the axis of rotation. But to assume that the disturbance from this source was exactly four times as great in the one case as in the other, and thence to deduce the result corresponding to an infinitely short magnet, appears to me to be a procedure scarcely consistent with the degree of accuracy aimed at. If this method is to give results capable of competing with those obtainable in other ways, it will be necessary to use a much shorter magnet; or, if that is not practicable, to devise some method by which the distance of the poles can be determined and a suitable correction calculated.

In carrying out the observations in the usual manner, it is necessary to measure the distance between a mirror and a scale. By using a double mirror with two scales and telescopes, MM. Weber and Zöllner avoid the principal cause of

* *Ber. d. Kön. Sächs. Ges. zu Leipzig*, 1880, ii. p. 77.

difficulty, *i. e.* the unsteadiness of the suspended mirror, all that is then necessary to know with accuracy being the distance between the two scales.

In using this and the three following methods great pains must be taken with the levelling of the earth inductor, since the deviation of the axis of rotation from the vertical (at least in the plane of the meridian) gives rise to an error of the first order with (in these latitudes) a high coefficient. In this respect it would be a decided advantage to carry out the experiments in a locality nearer to the magnetic equator (see "Account of Experiments to determine the value of the B.A. Unit in Absolute Measure," Phil. Trans. for 1882, p. 684). It is to be hoped that the measurements commenced by Weber and Zöllner will be carried to a successful issue, as it is only by the coincidence of results obtained by various methods that the question can be satisfactorily settled. At present no value in absolute measure of the B.A. unit or of the Siemens unit has been published as the result of their work.

III. *Method of Revolving Coil.*

This method, first, it would appear, suggested by Weber, was carried into execution by the celebrated Electrical Committee of the British Association*, and more recently by myself with the assistance of Dr. Schuster and others†. The greater part of what I have to say upon this subject has been put forward already in the papers referred to, from which alone the reader can form a complete opinion on the merits or demerits of the method as hitherto practised. On the present occasion I must take many of the conclusions there arrived at for granted, or at most give a mere indication of the nature of the arguments by which they may be supported.

Method III. differs from II. mainly in the fact that in III. the earth-inductor is, so to speak, its own galvanometer, the needle whose deflections measure the currents being suspended at the centre of the revolving coil itself instead of at the centre of another galvanometer-coil forming part of the same circuit. If, as in II., the inductor-coil were simply twisted through 180° when the needle passes its position of equilibrium, the disadvantages of the simplification would probably preponderate over the advantages. The diminution of effect due to the oblique position of the coil relatively to the needle (except at the moment of passing the magnetic meridian) would indeed be compensated by the diminished resistance of the complete circuit, and, as will presently appear, considerable advantage

* Brit. Assoc. Reports, 1862–1867. Reprint, Spon, 1873.

† Proc. Roy. Soc. May 1881, Feb. 1882; Phil. Trans. 1882.

would arise in respect of errors in the measurement of the coil; but an almost fatal uncertainty would be introduced from the influence of self-induction.

The important advantage of III., obtained, as I believe, without any really important sacrifice, arises only when the inductor is set into uniform rotation. In II., if the connexions were maintained without a commutator, the current in the galvanometer-coil would be alternating, and therefore unsuitable for measurement with a magnetic needle; but in III., although the current in the coil itself alternates, the reversal of the coil relatively to the needle causes all the impulses to operate finally in the same direction. When, therefore, the coil is caused to revolve in a periodic time small relatively to that of the free vibration of the needle, a steady deflection is obtained which varies inversely with the absolute resistance of the coil.

If we omit for the moment all secondary considerations, although some of them may not be without importance, the formula by which the resistance (R) of the revolving circuit is given in terms of the mean radius (a), the number of turns (n), the angular velocity of rotation (ω), and the angle of deflection (ϕ), runs

$$R = \pi^2 n^2 a\omega \cot\phi;$$

from which it appears that, in respect of errors arising from the measurements of the coil, this method is much superior to those hitherto discussed. There is only one linear quantity concerned; and the error committed in its determination enters but singly into the final result. Indeed we may say that in this respect no improvement is possible, unless it be in the direction of substituting for the mean radius of a coil of several layers some other kind of linear quantity more easy to deal with.

In requiring the absolute measurement of angle, II. and III. stand precisely upon a level.

The time of vibration in the experiments of MM. Weber and Zöllner was 17 seconds or 30 seconds—none too long relatively to the time (2 seconds) occupied in turning the inductor. If we suppose the coil to be uniformly rotated at the rate of, say, 2 revolutions per second, there would be 68 or 120 impulses upon the needle in the time of 1 vibration. It would no doubt be a great exaggeration to represent the increase of sensitiveness as being in any thing like this proportion, since by the method of recoil it is possible to make several observations of impulses during the time required for one observation of steady deflection. Nevertheless it cannot

be doubted that the advantage of III. in respect of sensitiveness is very considerable.

Experience has shown that there is no difficulty in controlling and measuring the rotation of the coil; but of course some auxiliary apparatus is required for the purpose. Against this may be set the escape from observations of the time of vibration, and from any uncertainty which may attach to the ballistic use of a galvanometer-needle. The suspended magnet may easily be made of such dimensions that no appreciable error can arise from supposing it to be infinitely small.

On the other hand, some new complications enter in method III. which I desire to state in full. In the first place we have to take account of the fact that the inductor moves in a field of force due not only to the earth, but also to the suspended magnet itself. I do not think that the correction thus rendered necessary (about 4 parts per thousand in my experiments) adds in any appreciable degree to the uncertainty of the final result; but we may take note of the fact that an auxiliary determination must be made of the ratio of the magnetic moment of the suspended magnet to the earth's horizontal force.

If the metal ring on which the wire is wound be on a large scale and sufficiently massive for strength, currents may be developed in it, even although it is divided into two parts by ebonite insulation. In my experiments the effect of these currents was very sensible, and had to be allowed for by careful observations of the deflection produced when the ring was rotated with wire circuit open. In any future repetition it will be worthy of consideration whether the ring should not be formed of less conducting material. It does not appear, however, that the final result can be prejudicially influenced; and the effect produced by secondary closed circuits allows us to verify the insulation of contiguous layers or turns of the wire by comparing the deflections obtained before the wire is wound with those obtained after winding, but with main circuit open, any difference being due to leakage.

But the most serious complication in method III., and one which in the eyes of some good judges weighs strongly against it, is the disturbing influence of self-induction. With respect to this, the first point to be noticed is that the action is perfectly regular, and that the only question which arises is whether its magnitude can be determined with such accuracy that the final result does not suffer. Now the operation of self-induction is readily submitted to calculation if a certain coefficient (L) be known. We find

$$R=\pi^2 n^2 a\omega \cot\phi\{1-U\tan^2\phi-U^2\tan^4\phi\},$$

where U is a numerical quantity dependent upon L, so that the influence of self-induction is approximately proportional to the square of the speed of rotation. The same law applies also to any disturbances depending upon mutual induction between the wire circuit and subordinate circuits in the ring.

It will be seen that, if the law of squares may be depended upon, the influence of self-induction (and mutual induction) can be satisfactorily eliminated by combining observations taken at different speeds. In my experiments four speeds were used, of which the greatest and the least were in the ratio of 2 : 1. The effect of self-induction was therefore four times as great at the high speed as at the low speed. In other words, the quantity (about 1 per cent.) by which the low-speed result is to be corrected in order to eliminate the influence of self-induction is only one third of the discrepancy between the uncorrected results of the extreme speeds. If, therefore, the observations are good for any thing at all, they are good enough to determine this correction with all desirable precision. If a check be considered necessary, it is supplied by the results of the intermediate speeds.

The above reasoning proceeds upon the supposition that we have no independent knowledge of the magnitude of the coefficient U. In point of fact, this coefficient can be calculated with considerable accuracy from the data of construction, so that the empirical correction is applied only to a small outstanding residue.

In considering the disadvantageous influence of self-induction as an argument in favour of II. as against III., we must remember that the magnitude of the influence can be greatly attenuated by simply diminishing the speed of rotation. At half the lowest speed above spoken of, for which the correction for self-induction would be reduced to $\frac{1}{4}$ per cent., the deflection (over 100 millim. at a distance of 2670 millim.) would probably correspond to a much greater sensitiveness than it is possible to obtain under II. If we prefer the higher speed, it is because we estimate the advantage of doubled sensitiveness as outweighing the disadvantage of a fourfold correction for self-induction.

The fourth objection which may be taken to this method, and it is one from which II. is free, lies in the necessary creation of mechanical disturbance in the neighbourhood of the suspended magnet.

How far these complications may be supposed to prejudice the result of carefully conducted experiments must be left to the estimation of the reader of my paper, in which very full data for a judgment are given. My own opinion is, that while

in the aggregate they must be allowed to have some weight, they are far from preponderating over the advantages which the method possesses in comparison with II.

If we take the view that the method itself is trustworthy, the principal error will arise in connexion with the mean radius of the coil; and it becomes an interesting question to consider whether advantage may be expected from a further increase in the dimensions of the apparatus. For this purpose we may regard $\tan\phi$ as given. The total resistance R will be proportional to n^2a/S, where S denotes the aggregate section of the copper, from which it follows that $\omega\mathrm{S}$ may be regarded as given, while a is left undetermined by the consideration of sensitiveness. Thus, if we retain ω and S unaltered in a magnified apparatus, we shall have the same sensitiveness as before, while the increased diameter of the coil and the relatively decreased dimensions of the section will conduce to a more accurate determination of the mean radius.

The angular deflection being given, the correction for self-induction is nearly constant whatever may be the proportions of the coil.

If we are of opinion that there is danger in the operation of self-induction, the case becomes strong for the introduction of a second coil in a plane perpendicular to that of the first*. By this means the relative correction for self-induction would be reduced to one quarter, while the deflection remained unaltered. It scarcely needs to be remarked that this use of a second coil would not, as in II., increase the uncertainty depending upon the linear measurements, the two mean radii entering into the result as parts, and not as factors.

This combination would lend itself especially well to low speeds of rotation; for the deflecting force, being uniform in respect to time, would not give rise to forced vibrations of the needle. The latter would have nothing further to do than to indicate the direction of a constant field of force.

IV.

This method, which was proposed by Foster †, and more recently by Lippmann, and to a certain extent executed by the former, is a modification of III., in which the electromotive force generated during the rotation of the inductor is balanced by an external electromotive force, and thus not allowed to produce a current. The external electromotive force is due to the passage of a battery-current through certain resistance-coils; and the current is compared with the earth's horizontal

* Proc. Roy. Soc. May 1881, p. 123.
† Brit. Assoc. Report, 1881.

intensity (H) by an absolute tangent-galvanometer. The difference of potential at the two points of derivation is thus known in terms of the included absolute resistance (R) and H. The circuit is continued through a sensitive galvanometer and the coil of the inductor, and is closed only when the latter coil is nearly in the plane of the meridian. When balance is obtained, the electromotive force of induction $n \cdot \pi a^2 \cdot \mathrm{H} \cdot \omega$ is equal to $\mathrm{RH} \tan \alpha / \mathrm{G}$, where G is the constant of the tangent-galvanometer and α the angle of deflection. The result, from which H disappears, if it may be assumed to be the same in the two places, is thus

$$\mathrm{R} = n\pi a^2 \mathrm{G} \cdot \omega \cot \alpha,$$

or, if A be the mean radius of the galvanometer-coil,

$$\mathrm{R} = 2n\pi^2 \omega \cot \alpha \frac{a^2}{\mathrm{A}},$$

from which the value of the resistance-coils is obtained in absolute measure. One advantage of this method, which it shares with VI. below, is that the resistance immediately expressed may be that of well-constructed coils of German silver or of platinum-silver at a known temperature.

This method is nearly free from the secondary objections to III. discussed above. The self-induction of the revolving wire-circuit does not enter, as no appreciable current is allowed to form itself; but there would appear to be a possibility of disturbance from mutual induction between the wire-circuit and secondary circuits in the ring. It would certainly be necessary to prevent the flow of currents round the ring by the insertion of an insulating layer; and even with this precaution some control in the way of a variation of speed would almost be necessary. Again, it is a question whether disturbance, from thermo-electricity for instance, may not arise at the place where the contacts are made and broken.

It is to be hoped that a complete series of observations may be made by this method, which certainly possesses considerable merits; but at best it remains open to the objection mentioned under II., with which in this respect it stands upon a level, *i. e.* that errors may enter from the measurements of both coils, the error of A entering singly into the result, and that of a entering doubly.

In respect of requiring absolute measurements of angle, there is nothing to choose between II., III., IV., and V.

V. Weber's *Method by Damping.*

This is the method followed by Kohlrausch * in his investi-

* Pogg. *Ann.* Ergänzungsband vi.; Phil. Mag. 1874, April and May.

gations upon this subject. It is founded upon II.; but in order to avoid the difficulty arising from the necessity of using a magnet small relatively to the coil in which it is suspended, no attempt is made to determine the constant from the data of construction. The inductor is connected with a sensitive galvanometer, and the constant of the latter is deduced from observations of the logarithmic decrement of the vibrations of the magnet when ths circuit is closed (λ), and when it is open (λ_0). The result, however, involves H the horizontal intensity, K the moment of inertia of the needle, as well as the time of vibration T. Expressed roughly, in the notation previously employed, it is

$$R=\frac{32a^4H^2T\lambda}{K}\cdot\frac{AB}{(A^2+B^2)^2},$$

where R is the resistance of the circuit composed of the inductor and galvanometer, A and B are the arcs of vibration in the method of recoil.

Interesting as this method is in some respects, I cannot but agree with Rowland in thinking that the final formula is enough to show that it cannot compete with others on equal terms, if the object be to obtain a result of high accuracy. The horizontal intensity itself is perhaps nearly as difficult to determine as absolute resistance; and the error thence arising doubles itself in the result. There is in addition the error of K. But even if H and K were not subject to error at all, I believe that the occurrence of the fourth power of the radius of the inductor is a fatal defect, and tends to explain the discrepant result obtained by Kohlrausch*. It is also worthy of note that the error of levelling enters twice as much as in II., III., and IV.

VI. Lorentz's *Method.*

This method, which, with the introduction of certain modifications not affecting its essential character, I am disposed to consider the best of all, was proposed and executed by Lorentz, of Copenhagen, in 1873†. A circular disk of metal, maintained in rotation about an axis passing through its centre at a uniform and known rate, is placed in the magnetic field due

* Oct. 1882.—It is very satisfactory to note that Kohlrausch (*Gött. Ges.* Sept. 1882) has recently detected an error in the value of the area of the windings of the inductor assumed in his previous calculations. Introducing the new value, obtained by an electrical process analogous to that described in Maxwell's 'Electricity,' § 754, he finds

$$1 \text{ B.A. unit} = \cdot 990\times 10^9.$$

† Pogg. *Ann.* cxlix. p. 251.

to a battery-current which circulates through a coaxal coil of many turns. The revolving disk is touched near its centre and circumference by two wires. If the circuit were simply closed through a galvanometer, the instrument would indicate the current due to the electromotive force of induction acting against the resistance of the circuit. The electromotive force corresponding to each revolution is the same as would be generated in a single turn of wire coincident with the circumference of the disk by the formation or cessation of the battery-current. If this be called γ, and M be the coefficient of induction between the coil and the circumeference, m the number of revolutions per second, the electromotive force is $mM\gamma$. For the present purpose, however, the circuit is not simply closed, but its terminals are connected with the extremities of a resistance R through which the battery-current flows, and the variable quantities are so adjusted that the electromotive force $R\gamma$ exactly balances that of induction. When the galvanometer indicates no current, the following relation, independent, it will be observed, of the magnitude of the battery-current, must be satisfied,

$$R=mM;$$

and from this, M being known from the data of construction, the absolute resistance R of the conductor is determined.

It will be seen that this method has pretty close affinity to I. The secondary circuit is here, in a sense, reduced to a single turn, or rather to as many turns as the disk makes revolutions in a time comparable with the time of swing of the ballistic galvanometer; but the disadvantage of a reduced number of turns is probably more than compensated for by the continuous character of the induced current, which allows of its being brought into direct opposition to that of the battery. During the months from April to August of the present year I have been occupied in carrying out a determination by this method. Space will not permit of a detailed consideration of the various questions which presented themselves; and I must content myself with a brief statement of the procedure, and with such a discussion of the sources of error as will allow a comparison of this method with others. I hope shortly to communicate a detailed paper upon the subject to the Royal Society.

One of the principal difficulties to be overcome arises from the exceeding smallness of the resistance R, less than $\frac{1}{200}$ B.A. in my experiments. Lorentz employed an actual column of mercury of known dimensions, so that the result is given at once in terms of mercury. I had intended to follow the same course, but, after some trials, came to the conclusion that there would be difficulties in the way of thus obtaining the

degree of accuracy aimed at, and ultimately adopted a method of shunting. The main current from the battery was divided into two parts, the larger of which passed through a resistance of half a unit, formed by combining two singles in multiple arcs. The resistance traversed by the other part of the main current was much larger (from 10 to 20); and it was to two points on this branch distant $\frac{1}{10}$ that the wires of the derived circuit were connected. With proper precautions this arrangement was found satisfactory, and the equivalent resistance R could be accurately expressed in terms of the standard B.A. units. The adjustment for obtaining the balance was effected by varying a large resistance placed in multiple arc with one of the others; or rather two effective resistances were used, one on either side of that required for balance, the latter being finally calculated by interpolation from the indications of the galvanometer.

By observing only the effect of reversing the battery-current the results are freed from the influence of terrestrial magnetism, and from the very sensible thermoelectric force having its seat at the sliding contact. These contacts were made by means of brushes of copper wire. One brush pressed against the cylindrical edge of the disk, which was about $\frac{1}{4}$ inch broad; and the other pressed against the shaft on which the whole turned. The area included by the secondary circuit was therefore not exactly that of the disk, but required a small correction, as to which, however, there is no difficulty.

The arrangements for driving the disk and for observing the speed were the same as for the revolving coil of method III. The results, which in the same arrangement have not differed by so much as $\frac{1}{1000}$ on different days, show that the sensitiveness was sufficient.

After these explanations I come to the main subject of the present remarks, viz. the degree of accuracy likely to be attained in the fundamental linear measurement. In the present case the quantity to be determined is M; and so far there is no difference between this method and I. But the fact that the secondary circuit is here represented by a disk whose diameter can be measured much more accurately than that of a coil introduces a certain modification. It is necessary also that the arrangements be symmetrical with respect to the middle plane of the disk, as, on account of the width of the brush, the place of contact cannot be considered as well defined. The necessary condition can be satisfied with a single coil by placing it so that its mean plane coincides with that of the disk. In this position slight errors of adjustment produce effects of the second order only, and every thing depends upon the radii.

Preparatory to the design of the apparatus for my experiments, I made some calculations of the values of the induction-coefficient and of its rates of variation for various ratios of the radius of the coil (A) to that of the disk (a). The angle γ (see method I.) is here ($b=0$) determined by $\tan^2\frac{1}{2}\gamma=a/\mathrm{A}$. If we write

$$\frac{\delta \mathrm{M}}{\mathrm{M}}=\lambda\frac{\delta \mathrm{A}}{\mathrm{A}}+\nu\frac{\delta a}{a},$$

the sum of λ and ν will be unity. The following are the values found. Those under M are proportional only, and relate to the case in which A is constant.

a/A.	λ.	ν.	M.
·5	−1·2	+2·2	4·37
·6	−1·36	+2·36	6·65
·7	−1·5	+2·5	9·80
·8	−2·0	+3·0	14·4

In Lorentz's apparatus the value of a/A was even larger than the last in the table, and the radial dimension of the coil was no small fraction of $(\mathrm{A}-a)$. On this account, as has already been pointed out by Rowland, no very accurate result could be expected.

In my experiments two similar coils were used whose radius (A) = about 26 centim., and in two distinct arrangements. In the first arrangement the two cells were placed close together; so that the case corresponded pretty closely with that just spoken of. The radius of the disk is about 16 centim.; and thus the proportions are nearly those of the second example in the table. It will be seen that the circumstances are not unfavourable to accuracy, the error of mean radius of the coil entering into the result to a less extent than in any of the methods hitherto described, except III. and IV. The disk is so much more easily measured, that the larger coefficient 2·36, applicable to it, should not lead to much error in the result.

This arrangement was worked at two speeds of rotation in the proportion of 10 : 16, and gave with close accordance

$$1 \text{ B.A. unit} = \cdot 9867\times 10^9 \text{ C.G.S.}$$

In the other arrangement the two coils were separated to a considerable distance, and the induction-coefficient depended not only upon the mean radii of the coils (and of the disk), but also upon the distance of their mean planes. The peculiarity of this arrangement, to which I wish to draw special

attention, is that it is possible so to proportion the quantities that *the error of mean radius of the coil does not affect the result*, which accordingly depends only upon the diameter of the disk and the distance of the coil's mean planes. How this may come about will be readily understood by considering the dependence of M upon A when a and b are given. It is clear that M vanishes, both when A is very small and when it is very large; from which it follows that there must be some value of A for which the effect is a maximum and therefore independent of small variations of A.

In carrying out this idea it is not necessary to approach the above-defined state of things very closely; for of course we have in reality a good approximate knowledge of the value of A. In my apparatus the distance of mean planes was about 30 centim., so that $b =$ about 15 centim. With the actual proportions a calculation of the effects of the various errors shows that

$$\frac{\delta \mathrm{M}}{\mathrm{M}} = \cdot 12 \frac{\delta \mathrm{A}}{\mathrm{A}} - \cdot 96 \frac{\delta b}{b} + 1\cdot 8 \frac{\delta a}{a};$$

so that the error of A enters in quite a subordinate degree. The positive coefficient of δA shows that with the given coils and disk the separation was somewhat too great to secure the greatest independence of δA.

The success of this arrangement depends principally upon the degree of accuracy with which b can be determined. The two rings on which the wire is coiled are separated by distance-pieces; and, as in 1., by reversing the rings relatively to the distance-pieces the result may be made to depend upon the mean length of these pieces and the mean thicknesses of the rings at the places of contact. The three distance-pieces were held together in one length and measured under microscopes; and the thicknesses of the rings were taken with verified callipers. There can hardly be a doubt but that this determination is much more accurate than that of the mean radius of a coil; and, what is also of some importance, it admits of repetition at pleasure with comparatively little trouble.

The value of the B.A. unit resulting from the measurement with this arrangement was $\cdot 9869 \times 10^9$ C.G.S.*

There seems no reason why a further increase of accuracy should not be obtainable by enlarging the scale of the apparatus. If we suppose the scale doubled, the number of turns in the coil and the angular speed of the disk being unaltered, the value of M would be doubled; and thus with the same

* The reductions not being yet finally completed, these numbers are liable to a change of one or two units in the fourth place of decimals.

battery-current the sensitiveness would be improved. Or, if we suppose the circumferential linear speed of the disk rather than its angular speed to be constant, the sensitiveness would be unchanged. If the larger coil were made of the same kind of wire as the smaller, its resistance would be augmented; but if the dimensions of the section were also doubled, so as to keep the proportions throughout, the advantage in this respect would lie with the larger apparatus.

On the whole, I am of opinion that if it is desirable at the present time to construct apparatus on the most favourable scale, so as to reach the highest attainable accuracy, the modification of Lorentz's method last described is the one which offers the best prospect of success. Before this is done, however, it appears to me important that the value now three times obtained in the Cavendish Laboratory by distinct methods should be approximately verified (or disproved) by other physicists. To distinguish between this value and those obtained, for instance, by Kohlrausch, by Lorentz, or by the first B.A. Committee, should not require the construction of unusually costly apparatus. Until the larger question is disposed of, it appears premature to discuss the details of arrangements from which the highest degree of precision is to be expected.

XXXIX. *On the Correlation of the Chemistry of the Carbon Compounds with the Phenomena of Life.* *By* C. F. Cross *and* E. J. Bevan*.

IT is not for us to dilate upon the marvellous progress of Organic Chemistry during this century, nor to find fault with the inevitably specializing tendency of research in the province of the carbon compounds; and we certainly owe an apology for entering upon a subject of such magnitude as the correlations of chemical with biological science. That which we offer is derived not so much from the consciousness of being able to originate views of these correlations which shall be more productive than certain which appear to prevail, as from the practical necessities of the investigations in which we find ourselves engaged, the paucity of the landmarks to which we have to look for guidance, and the misleading character of certain of the recognized principles and methods which has become manifest in the results of their application. In fine, there are numerous points in that portion of Biochemical Science the study of which we are prosecuting which call for critical discussion; and the existence of the imperfections

* Communicated by the Authors.

which it shall be our aim to expose we can only account for by the influence of the specializing tendency preventing chemists generally from following up the science in its wider relations.

It is certainly the ideal issue of organic chemistry to co-ordinate the multitudinous facts already and to be amassed concerning the carbon compounds, with the genesis, changes, and ultimate fate of the substances which go to build up the tissues of living organisms. Beyond this, indeed, many chemists do not hesitate to indulge in expectations as to the possible achievements of synthesis, which know no limits short of the inconceivable. The special "vital force" of a previous age they dismiss as an ancient cloke of ignorance long since discarded, under the genial influence of the sun of knowledge, even by those who most tenaciously opposed its sheltering folds to the stormy blast of "unbelief;" and thus by removing the great barrier, the whole universe of matter and force is opened out to the "conquering progress of man." In the words of an authoritative modern text-book, "At the present day the belief in a special vital force has ceased to encumber scientific progress. We now know that the same laws of combination regulate the formation of chemical compounds in animate and inanimate nature"*. The authors of this manifesto, however, leave us in doubt as to whether they regard the belief in question as itself obsolete, *or*, by being modified in accordance with the invincible array of facts by which their second dogma has been established, as brought within the pale of natural truth, and thus to have become an aid, and not an encumbrance, to scientific progress. The two propositions are certainly not equivalent unless this latter interpretation be allowed; and as equivalence is evidently intended, we take it that the authors, leaving the mystery of life to vindicate itself, also intended this interpretation, and would allow the chemical phenomena of life to be as special, say, as the phenomena of heat.

But it is difficult to rest satisfied with the isolated and physical interpretation of the passage; the generalizing tendency of modern physical science impels us to give it a wider consideration. Thus, to develop our parallel, we have long ceased to regard heat as having any special objective existence; and although its phenomena are, in relation to our perceptions, still sufficiently special to admit of classification apart, we no longer allow the exigencies of science to impede our progress towards a better understanding of the unity of nature, but recognize in heat "a manifestation of energy as a mode of molecular motion," a definition which is sufficiently exclusive

* Roscoe and Schorlemmer, 'Treatise on Chemistry,' iii. p. 10.

of subjective impressions. Further, chemists are in the habit of referring the phenomena of their science to the existence of a force of chemical affinity, and that without any justification more or less elaborate, in deference to its conditional character or to the metaphysical questionings which underlie all our natural science. And when we examine into the grounds upon which our belief in a vital force may be said to be dismissed, we find that we have in them only the basis of a truer knowledge than heretofore of what the vital force is and what it is not. Since the dismissal of the hypothesis of spontaneous generation, the distinction of matter into animate and inanimate has assumed a very sharp character. In the animate world we have a province of distinct phenomena; into this world, matter is coerced and is made to manifest properties distinct from those which it otherwise possesses; and in this world force is distributed and co-ordinated in such a way as to compel the acknowledgment of agency. Speaking physically, we admit that life is one of the narrowest—*i. e.* most extensively conditioned of phenomena; but this does not lessen our belief in the working, under these conditions, of a special agency. The minutely intimate correlation of life with its chemical phenomena doubtless leaves in the minds of many but a very narrow margin for the operation of the special agency in question, and makes its assumption appear proportionately gratuitous. At the same time we have no proof that the science of "energy" affords the ultimate criterion of natural truth; and we cannot recognize that it has done more than *modify* the belief in a special vital force, though the modification has been so deep as to convert it from being an encumbrance to an effective aid to scientific progress.

How far our progress, thus emancipated from a serious impediment, may be expected to go, is a question which must be relegated to metaphysics; at the same time we hold that the results of physical inquiry have as yet given no warrant for anticipating, as the realizable ideal, that our science will ever overleap the barrier of structural organization. The limitation herein expressed seems to us so obviously to define the *natural* attitude of the chemist towards living matter, that not without the strongest proofs will he change it for the extreme of conceivable ideals; and in the practical work of investigating the products of life and growth he will find such abundant confirmation of his natural impressions as to remain convinced that the distinction of the material universe into animate and inanimate is real and transcendental*.

* This subject will be found exhaustively discussed in 'Chemical Difficulties of Evolution,' by J. J. McLaren (1877).

Chemistry and biology occupy to one another an antithetic relation as regards their subject-matter : the goal of the one is the starting-point of the other; the protoplasm as yet undifferentiated, to which the biologist complacently refers the origin of life, is to the chemist a perfect microcosm, and its synthesis perhaps the highest possible achievement of his science. With regard to the supervening phenomenon of organization —the entrance of life—there is also a distinction of attitude. To the biologist it is axiomatic—beyond that, a fact without the range of observation, and one of which we venture to say, therefore, he can give no account. Now the chemist finds his attention challenged, and his work of speculative investigation begin, at an earlier point in the history of the planet, when as yet life was impossible ; his speculations moreover have a certain basis in observation and analogy; and having initiated his career, why break off and affect to view with veiled eyes a *change* merely in the disposition of the matter and force already existent and active? Why, further, should we attach to the entrance of structural organization a mystic significance, when not only are we familiar with the inverse change from the organized to the amorphous condition, but find it in many cases to involve no change in the inner subsensible molecular structure, or at least no deeper change than can be included within the province of isomerism? Cellulose, for instance (in the form of cotton), is dissolved by the ammonio-cupric reagent, and on adding excess of acid is reprecipitated, and may be recovered without loss of weight; the change undergone cannot, therefore, be more than morphological. Further, the amorphous cellulose behaves towards reagents so similarly to the original, that the change in question appears to have affected merely the external structure. Add to these considerations many others of a similar character, which need not be here specified, together with the numerous syntheses of the products of life and growth which have been achieved in the laboratory, and lastly the narrow range of physical conditions under which life is possible, and we have a fair conception of the intellectual position from which might emanate the dogma, "we have ceased to believe in a special vital force." We, speaking personally, see no more reason, in these teachings of molecular philosophy, for ceasing to believe in life as resulting from special agency, than for ceasing to recognize the living individuality of our English language or constitution, and the agency of Englishmen in their establishment and development, because they are originally a rearrangement of materials and forces once the possession of now obsolete or effete nationalities. We are not aware that in any philosophy,

worthy the name, life was or is regarded as any thing more than a rearrangement and special disposition of the preexisting: it cannot be doubted that life, as known on this planet, had a beginning; nor, further, that the whole analogy of the origin of subsequently derived life points to it as an individual and special phenomenon. In a word, the doctrine of Spontaneous Generation has been expunged from biological science; and a revival of its analogue from the point of view of chemical science we hold to be groundless. Not only so, but, to give a more practical expression to these views, a loose adoption of the non-belief in a special vital force will materially impede progress in the domain of biological chemistry. We cannot find a stronger proof of the want of recognition of the special and peculiar nature of the chemical phenomena of life than in the prevalence of empirical methods for the resolution of plant substances, their empiricism consisting chiefly in their unreasoned foundations upon the methods of separation of inorganic substances. To this portion of the science our attention has been specially directed; and we proceed to discuss it in certain of its bearings upon the main subject of this paper.

The work of the plant, considered in its intrinsic results, is to grow, to form tissues and organs; a side issue of growth is the elaboration of substances which subserve future growth; and a subsidiary result is the formation of certain substances which we may regard as excreta, as unavailable for the main end. These excreta are often bodies of well-defined physical properties, of more or less simple molecular structure, whose constitution is so far comprehended as to allow of their synthetical production in the laboratory. For the isolation of such bodies, methods founded upon the fixity of their constitution are general and satisfactory. But, on the other hand, substances whose essential condition is that of continual differentiation, whose constitution is but little understood even when viewed statically, whose relationship to the former group is probably of a parental character, should be treated with due regard to these distinctions: indeed it seems hopeless to attempt to comprehend their chemical functions in disregard of these their special biological correlations. The neglect of these considerations has led to the adoption of empirical and, to that extent, arbitrary methods of analysis and classification; names have been multiplied to individualize bodies which, occurring in a developmental series, should have received a corresponding general or group definition; and much time and capability have been spent in establishing facts before their probable and relative value had been taken into consideration.

The chemistry of cellulose is a case in point. This substance will be found, even to this day, treated by chemists as a well-defined body, of intrinsically fixed characteristics; whereas physiologists, with special regard to its functions in the living plant, have long since observed it to be capable of many modifications, more or less profound, and have not hesitated to regard it as the parent substance of that large class of aromatic bodies of which tannin is the type.

Geology had made us acquainted with the very profound modification of cellulosic structures which are seen in coal; and the chemical study of coal in its various forms had revealed a progressive increase in aromatic potentiality; and yet the obvious generalization of these several results has been widely ignored, the genetic connexion suggested by this large group of naturally occurring substances has been practically neglected, and chemists have remained satisfied with a purely empirical treatment. Taking this connexion as a working hypothesis and investigating the essential relations therein suggested, we feel assured that a much more productive field of inquiry is opened out. The essential difference between coal and cellulose is measured by the difference of their products of decomposition by dry distillation: in coal we have the source of the vast series of aromatic compounds which constitute the subject matter of the most important development of our science. Although the constitution of coal is still unsolved, and we cannot yet say to what extent it might be made to yield aromatic bodies by less drastic processes of resolution than that of destructive distillation, yet the general fact that in relation to cellulose it possesses an increased (immediate) aromatic potentiality has been sufficiently established. The nitrogen, further, derived from the proteid matters of the parent tissues exhibits a progressive diminution; and the chemistry of the formation and decomposition of coal may be considered independently of this element*. It remains therefore to investigate the conversion of cellulose into substances of the aromatic group of compounds.

The first link in the chain of development would appear, from the researches of physiologists, to be contained in the phenomenon of the lignification of cellulose structures. In the life of the plant, extreme processes of reduction and oxidation, of synthesis and resolution occur simultaneously and continuously. The formation of cellulose and its lignification have been ranged by physiologists on the basis of this antithesis. The connexion between lignin and the great group of

* W. A. Miller, 'Elements of Chemistry,' iii. p. 145, by whom this subject appears to be *developed* more consequently than by most writers.

astringent substances has been already established*; and the latter have been assumed by physiologists to be residues of the oxidation of carbohydrates. In regard to the lignification of structures originally consisting of pure cellulose, we have two alternative hypotheses to account for the change:—(1) that it results from an intrinsic modification of the cellulose itself effected *in situ*; and (2) that it is the result of combination of cellulose with aromatic bodies formed elsewhere in the plant, and probably as residues from the oxidation of carbohydrates. There is perhaps more negative evidence on the subject of the second than there is positive in favour of the first hypothesis; and the difficulties which beset the elucidation of the question are an apt illustration of the limitations which impede the solution of the chemical phenomena of life. If the cellulose combine with astringent substances presented in solution, to form insoluble products, these may be assumed to resemble those compounds which form the basis of the dyeing of the cellulose fibres. A superficial study of any of the non-cellulose or lignified fibres will satisfy the observer that they behave rather as modified celluloses than as a compound of the above weak order. In the first place, the resolution of the fibre-substance of lignified fibres can only be effected by means of drastic reagents, whereas a compound of cellulose with a body that it has merely removed from solution (that is to say, in what we may term adhesive combination) is always easily decomposed. We have further made a series of observations upon seedlings, which show that the astringent substances formed during germination are present in the juices and yet absent from the substance of the fibro-vascular bundles. Again, if lignification followed the course in question, it is difficult to account for the comparatively invariable composition of bast-fibres. Not only are they remarkably uniform in composition from end to end, but they may be obtained white and lustrous by a process of bleaching which occasions a minimum loss of weight; they may be converted into explosive nitro-compounds, an examination of which shows them to be homogeneous; they are soluble in the ammonio-cupric reagent, a property which has been denied to them by certain observers; and the precipitate obtained on adding an acid is simply the amorphous modification of the originally organized fibre-substance. The characteristic yellow coloration moreover which lignified fibres give when treated with a solution of aniline sulphate, and upon which much stress has been laid as distinguishing them from the

* Sachs, *Handb. der exper. Phys.* (1865) pp. 361–369; Sachse, *Farbstoffe* &c. (1877), p. 113; Cross and Bevan, Chem. Soc. Journ. xli. p. 90.

cellulose fibres, we have found to be so considerably weakened by previously boiling the fibre in a solution of acid sodium sulphite as to be afterwards in many cases quite undistinguishable. As the loss in weight due to this treatment is imperceptible, and the lignin substance remains practically unchanged, the colour-reaction in question is referable to the presence of some body resulting from a superficial decomposition (oxidation) of the lignin substance. These observations are a strong confirmation of the view enunciated with much emphasis by Sachs* many years ago, that the only inference to be drawn from the biochemical facts then established is that lignin and cellulose are genetically connected.

In our early work on bast-fibres, we were led to regard the jute-fibre as typical of a class of bodies analogous to the glucosides, and which we termed cellulides, a name which sufficiently explains our views. Subsequently to this, we found that Hlasiwetz† had arrived at similar conclusions in discussing the chemico-physiological relationships of the tannins, phlobaphenes, resins, and glucosides. He not only regards cellulose, tannins, and resins as genetically connected, but is convinced of the *à priori* probability of the existence of series of gummides and mannides parallel with the glucosides. We take this as an additional warrant for the correctness of our view; and in conformity with these conclusions and subsequent experience, we may state our hypothesis with more emphasis to be, that the fibre substance of lignified fibres is, in its chemical constitution, dominated by the cellulose molecule, upon which aromatic molecules, resulting from intrinsic modification of the cellulose itself, have been built. Whether the aromatic molecule is of the nature of a quinone, as would seem to be indicated by the products of the action of chlorine, we have some hesitation in affirming, recognizing more clearly than we then did the difficulty of reasoning from the products of decomposition of once living substances back to the condition under which they are formed.

The celluloses, which constitute the framework upon which the plant world is developed, being regarded thus as capable of modification, and lignification as the first of the series of changes through which they pass from the group of carbohydrates, to which they originally belong, to the extended range of naturally occurring substances of aromatic character included in the large group of astringents, in the several varieties of coal, and probably also of other important groups,

* Sachs, ibid. *Cf.* Koroll, *Quant. Chem., unters. Zstg. Kork, Bast* &c.: Diss., Dorpat (1880).

† Chem. Soc. Journ. xxxviii. p. 666; *Ann. Chem. Pharm.* cxliii. p. 40.

such as the resins, it is surely incumbent upon chemists to recognize the call to investigate the natural origin and history of the carbon compounds, and first of all in their relation to cellulose. We know the objection in the minds of many to forsake the familiar landmarks of positive physical definition, such as crystallization and molecular volume, for a province where the absence of the criteria hitherto regarded as all important makes the results to be obtained appear to that extent conjectural; we know indeed that the objection in many cases takes the more active form of almost refusing credence to any results obtained with substances that are amorphous and essentially transitional; and against this attitude a most emphatic protest is to be lodged. Arithmetic cannot cope with the physics of living matter; and we shall need to promote our equations and constants several degrees before we can include its chemical phenomena. Moreover the purity of substances, as the only condition in which to be approached by the chemist, will need a very elastic interpretation in presence of matter undergoing differentiation; and such properties as have hitherto been regarded as affording the only guarantees of purity will have no place in a vast amount of research that requires to be done. We clearly recognize the large amount of work already accomplished by isolated effort in this department of chemical science; but these remain for the most part uncorrelated, and, as a glance at the text-books will show, in a great measure unrecognized.

Not only have the suggestions of physiologists in regard to the probable origin of aromatic substances in the plant been but little developed by chemists, but the equally important correlation of the carbohydrates with the fats, which follows from their physiological equivalence, still lies without our science. Here also a transformation in series is suggested, the intermediate terms of which are probably to be found in cutin and analogous bodies, constituents of cork structures. The changes through which the transition is accomplished are probably very profound, more so than in the conversion of cellulose into lignin. Of the mechanism of the conversion we are as yet entirely ignorant; but we have the conviction, in this as in every other case, that the vital force of the plant operates through the same materials and forces which lie at our disposition, and that its results can therefore be studied and in some measure reproduced.

The study of these transformations must, in the first instance, of course be analytical; and most important correlations will follow from a comparative examination of the products of resolution of plant-substances. Take, for instance,

mucic acid, and represent its connexions by a diagram, and we find a large number of very important correlations implicated, thus:—

Cellulose.
|
Gums.
|
Mucic acid.
/ \
(Dichloro-muconic acid) (Pyromucic acid).
Adipic acid. Furfuraldehyde.

Cork tissues. Fats. Lignin. Astringents.

A quantification of these relationships, and a multiplication of investigations according to such *à priori* correlations, could not fail to establish important truths concerning the genetic connexions of the carbon compounds.

A further illustration of the want of correlation of chemical with biological investigation is to be found in the prevalence of empirical and statistical analyses of plant-substances in agricultural work. To a certain extent it is evident that statistical must prevail over molecular methods where the complex substances and mixtures of substances which are elaborated in the plant world are the subject of inquiry; but we contend that these may be ordered on a much more rational basis than is at present adopted.

The somewhat arbitrary choice of reagents, such as Schulze's for the isolation of cellulose, of boiling dilute sulphuric acid and alkali successively applied to plant-substances for the determination of their so-called crude fibre, the dismissal of whole groups of bodies as "extractive matters" or as "incrusting and intercellular substances," and the general absence of the recognition of the genetic connexion of these substances with those from which they are separated—in fine, the almost exclusive choice of the indirect and statistical before direct methods of observation, argues a certain misapplication of time and capability, and sufficiently accounts for the indifferent, if not critical, attitude of the greater number of chemists towards Biochemical science. We may cite the literature on the subject of chlorophyll, the proteids, the carbohydrates, including cellulose, the group of pectous substances, and, until the recent work of O'Sullivan and Brown and Heron, starch. A more special illustration is to be found in the work of Meissner and Shephard on the origin of hippuric acid in the urine of Herbivora. In order to identify the parent substance—that constituent of grasses which could yield the necessary benzoic

residue—the authors, adopting the statistical method of exclusive separation, arrived at length at the substance in question —a body which Foster*, in quoting their work, terms a form of cellulose. This substance was found to differ from cellulose, on the result of an aggregate elementary analysis, by a somewhat higher carbon percentage, such as, according to the reasoning of these authors, would be due to the presence of a body of the composition of quinic acid; and this they regard as a constituent of this substance, and as converted during the process of digestion, into hippuric acid. Pushing then the statistical method of inquiry beyond its limits, and evidently for the purpose of confirming *à priori* views, the authors appear to us to have missed the most important development of their otherwise valuable work. Had they examined the "form of cellulose" at which they arrived by a direct method of proximate resolution, they would have obtained the aromatic substances, allied to the astringents and phenols, which we have obtained from lignified fibres. That they are digested by the Herbivora has been established by numerous observers†; that they are the source of the benzoic residue necessary to form hippuric acid is *à priori* very probable: in fact it must be regarded as first in the order of the probabilities to be investigated. As such, indeed, it is occupying our attention; and whatever be the result it will be more valuable, because more definite, than any conclusion arrived at by means of the indirect method.

We refrain from extending our discussion of these subjects in anticipation of the more detailed publication of our researches. We think we have shown grounds for our statement that there is a general want of correlation of chemical with biological research, especially in the hesitation on the part of chemists to adopt, as working hypotheses, the wider-reaching conclusions of physiologists as to the natural origin and history of the carbon compounds. We have also endeavoured to show that, while our conception of the vital force has been modified so as to have entirely lost the significance that belonged to it in a previous age, we have no ground for dismissing it for the alternative view of life as immanent in the universe of matter and force. We have expunged an error that was partial, and are in danger of generalizing the negative by which it needed to be met.

Postscript.—Since writing the above, our researches have

* *Die Hippursäure* (Hannover, 1866); 'Text-book of Physiology,' 2nd edit. p. 354. *Cf.* Weiske, *Zeitschr. Biol.* xii. p. 241.

† Weende, *Berichte.*

established a connexion of the closest order between the aromatic portion of the molecule of lignin (bastose) and the triatomic phenols—a fact which considerably strengthens the views advanced by physiologists as to the correlations of the carbohydrates with the aromatic group, and the reasoning by which we have sought to emphasize them. The researches in question, on this point, will be published in due course.

XL. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* By Oliver J. Lodge, *D.Sc.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE discussion which has been carried on in your pages* respecting the dimensions of a magnetic pole serves to illustrate the divergency of thought between those in this country who have been brought up, electrically, under Faraday and Maxwell, and the continental philosophers so eminently represented by Prof. Clausius. From one point of view the discussion may be said to have been roused by a simple mare's nest constructed by dropping a factor out of one side of an equation (as was pointed out at once by Prof. Fitzgerald and by Mr. J. J. Thomson); but from another point of view it is the natural and inevitable consequence of the different aspects from which these matters can be regarded:—the English standpoint, in which the medium is recognized as the active agent, and is continually present both in the mind and in the formulæ; and the continental standpoint, from which the medium is perceived as so much empty space, and is taken account of as such in the formulæ. Both these aspects of the subject are worth consideration; and it may be conducive to future clearness to discuss them at moderate length.

Coulomb's measurements provisionally established the fact that in air the mechanical force between two electrically charged bodies was proportional to ee'/r^2; but the subsequent researches of Faraday proved that this proportionality only holds so long as the medium enveloping the bodies is unchanged, and that the above quotient must be multiplied by different factors in order to give the force exerted in different media. Thus if the same two charged bodies were immersed in bisulphide of carbon, they would repel one another with much less vigour than they do in air.

Introducing therefore as a factor the electric inductive

* Phil. Mag. [5] vol. xiii. pp. 376, 381, 427, 429, 431, 530; and vol. xiv. pp. 124 & 225.

capacity K, we have the general equation

$$F = \frac{ee'}{Kr^2}.$$

Now all that is solemnly essential with respect to the dimensions of the quantities here involved is that e^2/K must be a force into an area, or that

$$[e] \equiv [L][KF]^{\frac{1}{2}}.$$

If we proceed to define the unit of electricity so as to make K of no dimensions and to equal 1 for air, that is a convention, and it is the basis of the electrostatic system ; but the above statement is no convention, but a natural truth.

Precisely the same kind of thing is true in magnetism; and we now know that the force between two magnetic poles is not independent of the medium surrounding them, but that if the torsion-balance were full of, say, ferric chloride, the force between the two poles would be measurably less than if it were full of mere air. Thus we again need a factor for completeness ; and the real law is that

$$F = \frac{mm'}{\mu r^2},$$

or that

$$[m] \equiv [L][\mu F]^{\frac{1}{2}}.$$

The conventional *magnetic* system of units is based on the definition of m in such a way that μ, the magnetic inductive capacity of the medium, shall be of no dimensions, and shall for air be simply 1.

All then that is objectively and physically fixed about the matter is that the dimensions of $e/\sqrt{K}$ and of $m/\sqrt{\mu}$ are absolutely and mechanically definite, being each of them a length into the square root of a force, or

$$M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}.$$

But observe that the two system of units, the electrostatic and the magnetic, the arbitrary definition of e and the arbitrary definition of m, are in their origin utterly independent ; not that they are *unrelated*, but their relation must be a matter for future and experimental investigation. All we can so far say about them is, that, in every possible consistent system that can be adopted,

$$[eK^{-\frac{1}{2}}] \equiv [m\mu^{-\frac{1}{2}}] \equiv [LF^{+\frac{1}{2}}] \equiv [Mk^{-\frac{1}{2}}]. \quad . \quad . \quad (1)$$

(The last term of this triple identity is added for the sake of completeness, though it does not directly belong to the present

subject: the letter k is intended for the gravitation-constant as determined by the Cavendish experiment. I am not aware whether the question of the possible dependence of this constant on the optical density of the medium surrounding the attracting masses has ever been considered; but I feel sure that a direct experimental attack on this question would not be uninteresting, and it might lead to important results.)

We now come to the Œrsted-Ampère discovery of the connexion between m and e—the form of the connexion being that an electric current flowing in a closed circuit can produce a magnetic potential, and therefore of course can act on magnets, precisely as if it itself were a magnet of a certain strength and form. The potential so caused at any point in air is found to be simply proportional to the strength of the current and to the solid angle which the circuit subtends as seen from that point; or, in other words, the moment of the magnet which is equivalent to the current is simply proportional to the strength of the current and to the area of the contour round which it flows.

The unit of current most simply and directly applicable to these electromagnetic phenomena is not the old electrostatic unit at all, but a new unit which may be defined in many ways—as, for instance, these:—

The electromagnetic unit of current is that which produces unit magnetic potential at a point whence its circuit subtends unit solid angle;

It is also that which produces unit magnetic intensity, in a given direction, at a point whence the solid angle subtended by its circuit is changing at unit rate, per unit displacement, in that direction;

And, again, it is that current which when flowing round a contour of unit area is equivalent to a magnet of unit moment,—

all these statements being derived directly from the unit magnetic pole thus:—

Unit magnetic potential is defined to exist wherever a solitary and stationary unit pole would possess unit energy;

Unit magnetic intensity exists wherever unit pole would experience unit force; and

Unit magnetic moment is that possessed by two unit poles of opposite sign rigidly connected by a bar of unit length.

(The connexion between the old electrostatic unit and this new electrical unit thus magnetically defined may be expressed, if I am not mistaken, by saying that a ring charged with the electrostatic unit of electricity would have to revolve in its own plane with an angular velocity of about 3×10^{10}

radians per second in order to produce the same magnetic effects as the electromagnetic unit of current flowing in the same ring.

Or, conversely, the electrostatic unit magnetic pole would be that which would experience unit force if placed at the centre of a circle of unit radius in which the electrostatic unit of electricity was moving with unit velocity.

This definition I believe to hold equally well in any homogeneous medium; for it is pointed out below that the electromagnetic effect of a current is independent of μ; while as regards K, a quantity which we might perhaps think would be likely to affect the result, we must remember that electric displacement is totally independent of any such circumstance. So, corresponding to the common electrokinetic equation,

$$\text{Force} = m\int\frac{\mathrm{C}ds}{r^2},$$

we shall have, for a moving charge,

$$\text{Force} = \frac{mev}{r^2},$$

whence

$$[me] \equiv [\mathrm{ML^2T^{-1}}]. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If statements like these are in the main correct, and after the experiments of Rowland we are bound, I suppose, to believe in the truth of something of the kind, they ought to remove Dr. Everett's objection (Phil. Mag. June 1882, p. 431) as to the introduction of electrostatic units into magnetism; unless indeed he maintains the thesis—no doubt a tenable one—that directly you begin to carry a charged body about, the discussion of its performances no longer belongs to electro*statics*.)

Returning from this digression, we have now to ask whether the statements above made are really definite and independent of the magnetic properties of the medium surrounding the circuits, or must we introduce a factor to express the influence of this medium when it is other than air?

Mr. J. J. Thomson has instructively raised this question (Phil. Mag. for June); and he and others at Cambridge consider it a matter to be settled by experiment; and they further consider that, in order to agree with Maxwell's view, experiment ought to make the magnetic effect of a solenoid and its air-equivalent magnetic shell differ, as soon as they are both introduced into some medium for which μ is not unity. Now, though agreeing with this as far as it goes, I venture with diffidence to think that Maxwell would have drawn a distinc-

tion between the medium inside the region of the solenoid corresponding to the substance of the magnetic shell, and that outside. He over and over again lays stress upon the fact that artificial solenoids can only be compared with magnetic shells for the space outside the shells, and that the line of integration must never be allowed to thread the circuits. Let us follow this out and see what it means when applied to the above question.

I will assume it possible (for it certainly is theoretically possible) to imitate any steel magnet whatever by a proper arrangement of electric circuits, both being at present immersed in a non-magnetic (*i. e.* non-*magnetizable* or $\mu=1$) medium. The two arrangements are completely equivalent for all the region outside both—*the region outside both being defined by the shape of the steel.* For the comparison is not to be urged within the steel, because of the magnetized surfaces, which would have to be cut through, a circumstance which would completely alter all the conditions ; and it is not to be urged within the region of space near the solenoid which is the counterpart of the steel-occupied region, simply because here there are *no* magnetized surfaces to be cut through, and therefore the conditions will be continuous.

Now take some non-magnetic medium, which for shortness I will call "clay," mould it into the shape of the steel, and place it in or around the solenoid so as to mechanically define the limits of the "outer region." And now immerse both magnet and solidified solenoid in any medium for which μ differs from unity: I venture to assert that the equivalence which existed in air will be entirely maintained in the magnetic medium—even though that medium be iron or bismuth, —and that, for both, the magnetic intensity at any point will be its air-value divided by μ.

Still keeping both the things in the magnetic medium, remove the clay from the solenoid and permit the medium to flow into the space it occupied. If what I said before is true, the solenoid will now be too strong for the magnet, for the magnetic permeability of the interior will increase its effect μ times, while that of the exterior will, as before, diminish it $\frac{1}{\mu}$th ; so that the effect of the solenoid completely immersed in the medium is precisely the same as it was when in air, while the effect of the magnet, from whose interior the magnetic substance is of necessity excluded, is still $\frac{1}{\mu}$th of what it was in air. This latter seems to be the kind of experiment which Mr. J. J. Thomson suggests in his June letter (p. 429), and

which, he says, Mr. Sargant then intended (and I hope still intends) to carry out.

The solenoid being now completely surrounded with homogeneous magnetic substance, wall a portion of it in with paper or glass to the shape of the steel of the magnet, and then pull both magnet and solenoid out into the air again. Naturally the solenoid will still be μ times too strong for the magnet, but no further discrepancy need be expected; and if the current of the solenoid has been weakened when inside the magnetic medium so as to restore the disturbed equivalence, they will remain equivalent when the region external to both is again filled with common air.

These statements, if in their essence granted, require to make them complete certain provisos about the boundary of the vessel containing the magnetic medium, unless it be infinitely large, and also a discussion of what happens in the case of more than one magnetic medium. But the magnetization of bounding surfaces, and the accidents which happen to lines cutting surfaces of discontinuity, are perfectly understood and need not be here entered into.

Moreover, in making these statements I am merely saying what one would *expect* to happen without evidence to the contrary; but I am not for an instant implying that direct experimental investigation is unnecessary and would not be highly desirable. On the contrary, I think it would be most desirable and satisfactory to have the matter thoroughly sifted.

Supposing, then, that I have so far made no mistake, we can make the general statement of the equivalence of a current and a magnet thus—

The magnetic moment of a circuit is equal to the strength of its current multiplied by its effective area and again multiplied by the magnetic inductive capacity (or permeability) of the medium in the interior of the region enclosed by the contour (which region for a simple plane circuit is a mere shell), but is wholly independent of the magnetic properties of all the rest of the surrounding medium.

The corresponding dimensional equation is

$$[m]\equiv[\mathrm{L}\mu e\mathrm{T}^{-1}].$$

Substituting in this the value of $[m]$ from (1), we obtain the relation

$$[\mu\mathrm{K}]\equiv\frac{1}{v^2}, \quad \ldots\ldots\ldots \quad (3)$$

whence

$$\left[\frac{e}{m}\right]\equiv\left[\frac{\mathrm{K}}{\mu}\right]^{\frac{1}{2}}\equiv[\mathrm{K}v]\equiv\frac{1}{[\mu v]}. \quad . \quad (4)$$

These relations must all hold in any consistent system of units, since they express physical truths; but of course they are not all independent. The number of independent relations must be limited by the number of fundamental experiments, viz. three—Coulomb, Coulomb, and Oersted; and the shortest way of writing the independent relations is this:—

$$\left.\begin{array}{l}[\mu e^2]\equiv[\mathrm{K}m^2]\equiv[\mathrm{ML}]\\ \text{and}\quad [\mu \mathrm{K} v^2]\equiv 1.\end{array}\right\} \quad . \quad . \quad . \quad . \quad (5)$$

The electrostatic convention makes $[\mathrm{K}]=1$; the electromagnetic convention makes $[\mu]=1$.

So far every thing being pretty clear and straightforward, we have now to ask how it was that Prof. Clausius should have jumped to the conclusion that Maxwell had fallen into error*, or else that he held a theory of magnetism different from (and not merely an amplification of) Ampère's and Weber's†. With this latter horn of the dilemma, by the way, he is half allowed by Mr. J. J. Thomson (September) to have succeeded in transfixing Maxwell; and Dr. Everett (June) is not extremely energetic in his repudiation even of the alternative of the first.

Now it is perfectly true that Maxwell, in stating the current theory of magnetism, says without any kind of retraction or hesitation that "the magnetic action of a small plane circuit at distances which are great compared with the dimensions of the circuit is the same as that of a magnet whose axis is normal to the plane of the circuit, and whose magnetic moment is equal to the area of the circuit multiplied by the strength of the current. . . . . and if a magnetic shell . . . . &c. be substituted . . . . then the magnetic action of the shell on all distant points will be identical with that of the current." And in discussing Ampère's theory, he ignores the existence of magnetic media whose μ does not equal 1 as completely as Prof. Clausius could wish.

But then, according to Weber's extension of Ampère's theory (an extension I suppose universally accepted), the properties of magnetic substances of all kinds are explained by molecular electric currents, and no magnets or magnetic substances other than those consisting of current-conveying molecules exist. [And with reference to a remark of Mr.

* Phil. Mag. June, pp. 387 & 392. † Ibid. August, p. 126.

Thomson's in the September number, p. 225, I may say in passing that it seems to me that Maxwell held, though no doubt tentatively and hypothetically, the view that electric currents and small magnets are *identical* and not only *equivalent*.]

The coefficient μ is thus foreign to Ampère's theory applied universally; and this is how it has happened that Prof. Clausius has failed to recognize its existence and has been led into error*. A system dealing with Ampèrian magnets in media for which μ does not equal 1 is a mongrel combination which may no doubt be occasionally convenient but which never can be thoroughly satisfactory.

We may accept then without hesitation Clausius's presentation of Maxwell's views, viz. both that a small magnet *is* an electric current, and that *magnetic moment* ALWAYS *equals simply integral current* $\times$ *area*—remembering, however, that there exist currents in molecules besides the gross and artificial currents in our copper wires, that these are directed by our artificial currents and add to their effects, and that in all cases they are most distinctly to be taken into account.

In air, so far as it is non-magnetic, these molecular currents are zero, and the magnetic induction and the magnetic force are everywhere equal; but in media consisting of Ampèrian

* Very many errors, I now find; for he has also ignored K, Faraday's simple old electrostatic constant; and accordingly his equations (1), (2), (4), &c. express mere conventions (if they were any thing more, then truly m_s would have to equal m_d, and $e_s = e_d$, as he begins to perceive in his August letter); while his general equation (3), which is the foundation of his reasoning, is quite wrong, and is indeed at the bottom of the whole confusion. In using the term "error" here, I would be understood to mean rather "divergency from opinions commonly held in this country" than absolute incorrectness as to matter of fact. For it would not be becoming to apply the latter term to views held by Prof. Clausius when the experimental foundation of opposing views is confessedly incomplete. The views held by Prof. Clausius are no doubt perfectly consistent, and would probably be in accord with fact if only the medium produced no effect such as it is here commonly supposed to produce; and whether the medium does or does not produce such an effect appears to some extent at present a subject of legitimate debate and a matter for experimental investigation. It will be understood therefore, that in stating one side strongly I have been influenced by the wish to be clear, rather than with the desire to dogmatize.

Since the above letter was in type Dr. Francis has kindly called my attention to a paper by Prof. Helmholtz in Wiedemann's *Annalen*, No. 9, 1882, to which I might have further referred if I had known of it in time. As far as I can hurriedly understand his position, Prof. Helmholtz in part endeavours to reconcile the views of Maxwell and of Clausius by throwing a doubt upon the Weber-Ampère theory; and in fact he appeals to pure physicists not to abandon the old electrostatic for the more cumbrous and less surely founded electromagnetic system.

[A translation of Professor Helmholtz's paper will appear in our next number.—ED. Phil. Mag.]

molecules there is an extra magnetic induction, due to the pointing of these along the lines of force, which is 4π times the magnetization, and which has to be added to the other, thus making the total magnetic induction at any point μ times the magnetic force there.

The effect of the medium is a physical fact; and no theory can presume really to dispense with the constant μ. All that the Ampèrian theory does is to give a physical interpretation to it, and to render one independent of it so soon as one takes account of every current-conveying circuit, whether molecular or other, existing in the field, and does not arbitrarily elect to deal only with those gross solenoids which we can excite and immediately control by batteries.

There can be no doubt, I think, that the mind of Maxwell on this subject, as on most others, was as clear as daylight; and so far from falling into the least suspicion of an error, he expresses himself in art. 615 (1st edit.) almost as if he were joining in the present discussion, saying :—

"There is one result . . . . which is of very great importance. If we suppose that no magnets exist in the field except in the form of electric circuits, the distinction which we have hitherto maintained between the magnetic force and the magnetic induction [and therefore also the difference $\mu-1$] vanishes, because it is only in magnetized matter that these quantities differ from each other. According to Ampère's hypothesis . . . . . the properties of what we call magnetized matter are due to molecular electric currents, so that it is only when we regard the substance in large masses that our theory of magnetization is applicable; and if our mathematical methods are supposed capable of taking account of what goes on within the individual molecules they will discover nothing but electric circuits, and we shall find the magnetic force and the magnetic induction everywhere identical. In order, however, [N.B.] to be able to make use of the electrostatic or of the electromagnetic system of measurement at pleasure we shall retain the coefficient μ, remembering that its value is unity in the electromagnetic system."

I am, Gentlemen,

Your obedient servant,

OLIVER J. LODGE.

University College, Liverpool,
September 28, 1882.

XLI. *On the Electric Discharge in Rarefied Gases.* *By* Dr. EUGEN GOLDSTEIN*.

I.†

I HAVE shown in two former papers ‡, and more completely in my book 'A New Form of Electrical Repulsion' (published by Springer, Berlin), that the discharge cannot be effected by the actual projection of gas-particles. The same considerations which oppose the theory of the propagation of electricity by projected gas-particles, also at once exclude the assumption that other ponderable particles, having access to the space in which the discharge takes place, play any essential part as carriers of electricity in the discharge. Such particles might consist of disintegrated portions of the substance of the electrodes, particles of the wall of the vessel, or of dust.

The theory that the kathode-rays at any rate are produced by projected particles of the substance of the electrodes has been recently defended by Gintl§ and Puluj‖. Numerous arguments¶ may be urged against it, over and above those which are at the same time opposed to a special convection by the particles of the gas. I will briefly mention one or two points. I have mentioned on a former occasion** that a system of pores in an insulator, or a single opening of relatively small dimensions, sends out rays whose properties are equivalent to those of the rays which issue from a metallic kathode. The rays of narrow openings, for example, possess the property of rectilinear radiation, and of exciting phosphorescence, which cannot here be explained by a projection of the substance of the pole.

Kathodes imitated by systems of pores represent special cases of the phenomenon of secondary negative light, which is

* Translated from the *Annalen der Physik und Chemie*, 1881, new series, vol. xiii. Communicated by the Author.

† The readers of this Magazine will find that some observations and conclusions in the first chapter of the above paper, concerning the conductivity of vacuum, do not differ from the views expressed by Prof. Edlund in a paper reprinted in the January number of the Philosophical Magazine for 1882. I beg to mention therefore, that my paper appeared in print in the February number (1881) of Wiedemann's *Annalen*, and that Prof. Edlund presented his to the Royal Swedish Academy, April 23, 1881.

‡ Phil. Mag. [5] x. pp. 173 & 234.

§ Gintl, 'Studies of Crookes's Radiant Matter' (Prague, 1880).

‖ Puluj, *Sitzungsberichte Wien. Akad.* 1880, p. 864.

¶ E. Wiedemann (Wied. *Ann.* x. p. 252, 1880, Phil. Mag. x. p. 418) thinks it possible to conclude from Zahn's experiments (Wied. *Ann.* viii. p. 675, 1879) that the view of Gintl and Puluj is untenable.

** Phil. Mag. x. p. 177.

produced with the properties of rectilinear propagation, excitation of phosphorescence, &c., at openings of any width, if these openings occur in diaphragms or in tubes connecting vessels or introduced into them, the area of which is considerable in comparison with the diameter of the opening. A consideration of these bundles of rays possessing the properties of the kathode-light issuing from wide openings also protects the experiments on narrow openings against the objection that the coincidence of their rays with the kathode-rays depends on a conductive action of particles of the insulating substance which might possibly be torn off from the edges of the opening.

It has further been shown* that the positive light also possesses the property of rectilinear propagation, and of exciting phosphorescence when the exhaustion has been carried far; it does not seem to be reasonable to adopt an explanation for the phenomena of the kathode-light the principle of which is not applicable to the exactly similar properties of the positive light.

The most convincing proof, however, is given by the following observation, which I have made in experiments on the kathodic deflection. This last name was proposed in the book referred to above for the deflection of the electric rays there described, of which I have made use† in determining the velocity with which the electricity propagates itself. I may assume it to be known that a kathode of aluminium produces no deposit on the walls of the tube even after several hours' use, whilst a kathode of platinum of no great thickness soon produces a completely opaque metallic deposit on the part of the tube played upon by the kathode-light. Two straight smooth wire electrodes, *a* and *b*, are inserted in the end of a cylindrical discharge-tube parallel to the axis of the tube. If both are made at the same time kathodes of the same discharge, then each of the two wires causes a deflection, in the rays of the other which pass by near to it, of the nature of a repulsion. We have then, as already described, two sharply-bounded surfaces, of which the one receives no rays from *a*, whilst none of the electric rays emitted by *b* fall upon the other. At the density of gas favourable to the production of phosphorescence, both surfaces are distinctly seen upon the surrounding brilliantly phosphorescing surface. This phenomenon is also observed when one of the kathodes *a* is of platinum, until the increasing thickness of the platinum-deposit prevents the phosphorescence of the wall.

According to the view of Gintl and Puluj, that portion of the glass wall on which no rays fell from the platinum elec-

* Phil. Mag. x. p. 236. † Ibid. p. 246.

trode ought evidently to remain free from the platinum deposit. If, however, we examine the glass wall, we find that the surface from which the kathode-rays are deflected—as determined at high pressures by the visibility of the blue rays themselves, and at low pressures by means of the phosphorescence which they excite—*is just as thickly covered with the platinum as the surrounding portion of the wall*, and exactly as we observe to be the case in cylinders where the platinum wire alone acts as kathode while *b* is not excited. It follows that the rays of the kathode-light are deflected, but not the projected particles of the electrode; the two cannot therefore be essentially connected.

The discharge is therefore not to be explained by a projection of material particles, either of the substance of the electrodes or of the gas. It follows from the experiments on the order of magnitude of the velocity of propagation of electricity, taken together with the views held on the constitution of gases, that the assumption of oscillations of these particles does not afford a satisfactory explanation, and the assumption of motions of rotation remains equally unfruitful. The wall of the containing vessel is not altogether neutral in the passage of electricity through the space enclosed by it; it shows itself phenomena of charge and discharge which appear to be not altogether without influence upon the main discharge between the metallic electrodes. If we wish to go so far as to ascribe to particles possibly torn off from the walls of the vessel when they are discharged the same function which, in the view just considered, gas-particles or electrode-particles were unable to perform, this assumption, quite apart from all new objections, is open to all the objections urged against the previous hypothesis.

Recent investigations have shown that the dust suspended in gases plays an important and previously unsuspected part in the loss of electricity suffered by feebly-charged conductors in the open air, or in gases not specially purified. In almost all cases in which we have hitherto regarded a mass of gas as a carrier of statical electricity, we must now regard the dust suspended in the gas, in cases where drops of liquid cannot be present, as the only vehicle of the electricity. We might easily imagine an hypothesis ascribing a similar essential function to the dust in gases in the case of current electricity as in the case of statical electricity, but that the objections we have previously considered might easily be employed to refute an assumption of the kind.

The discharge cannot then in general be explained by the motions of ponderable particles; it follows therefore directly

from the experiments which prove this, that that medium must be essentially concerned in the discharge which, according to our present views, together with the gas-molecules, the particles of the electrodes of the walls, and any other solid substances which may be present, occupies the space in which the discharge takes place—that is to say, the æther.

According to my view, the discharge is a process which takes place *in the free æther.* I have already indicated this view in the work already several times mentioned, and will now give other evidence supporting the observation made there.

Hittorf* found that the resistance of the positive light always decreased as the exhaustion of the gas increased; on the other hand, he thinks he has shown that the resistance increases with the exhaustion in the kathode-light and at the surface of the kathode. Changes in the form and magnitude of the anode have no influence on the resistance. The great resistance which offers itself to the discharge at an extreme exhaustion, and finally leads to its extinction in a vacuum as perfect as possible, depends therefore altogether upon the resistance at the surface of the kathode and in the space filled by the kathode-light. After I had recognized that the peculiarities of the negative light might be produced at any point whatever of the column of positive light by simple changes in the cross section of the discharge-tube, and that each separate stratification of positive light is nothing else than a modified bundle of negative light, this opposition between kathode-light and positive light appeared to me just as doubtful as already a number of other supposed differences between the two, which I had found not to exist.

I found in fact that, exactly as with the positive light, *the resistance of the kathode-light at small pressures becomes vanishingly small in comparison with the total resistance of the discharge.* Hence, since no specific resistance exists at the anode, and since further, as already mentioned, the resistance of the positive light vanishes in comparison with the total resistance of the discharge, it follows that *the resistance to the discharge at very small pressures takes place entirely at the surface of the kathode.*

My experiments on this subject were not made by means of a galvanometer, like those of Hittorf, but by means of the spark-micrometer, which is here much more efficient. The spark-micrometer was included in a second circuit connecting the electrodes, and the distances of the poles of the micrometer compared for the different densities of gas and lengths of

* Hittorf, Pogg. *Ann.* cxxxvi. p. 1 (1869).

kathode-light at which the currents of the inductorium either cease to pass through the exhausted tube or else pass no longer through the micrometer*.

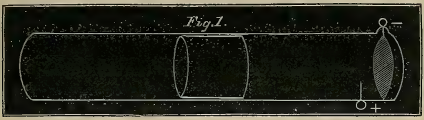
* The distances of the balls of the spark-micrometer corresponding to these two alternatives are not exactly identical. The discharges do not pass exclusively through the exhausted tube up to a certain distance of the balls, and then with a certain small decrease of this distance exclusively across the air-space between the balls; but between the distances of the balls at which the spark takes one only of the two courses open to it are to be found positions of the micrometer at which both paths are taken —sometimes the one, sometimes the other—and the one path the less frequently the nearer we come to the distance at which the other is taken alone. This apparently unstable character of the resistance in the tube does not affect the accuracy of the measurements here in question; we may compare the resistances by comparing the distances of the balls apart at which for a certain fixed time, say two minutes, no spark passes in the tube or between the poles of the micrometer. The distances so measured agree upon repetition to $\frac{1}{60}$ millim.—that is to say, to a fraction of 1 per cent. of the distance measured. The alternation of the discharges with certain distances of the balls no doubt depends, partly at least, upon the same cause as the following phenomenon, which I have observed in the same experiments, and which, as far as I know, has not been previously described.

If we include the spark-micrometer in a branch circuit of an exhausted tube which transmits both the discharge at "make" and that at "break," then, if the distance between the balls of the micrometer be gradually diminished, the current at "break" completely leaves the tube and passes altogether in the free air, whilst that at "make" continues to traverse the tube with undiminished light. Consequently we are able to study the discharge of the "make" current in the exhausted gas separate from that of the "break" current; whereas hitherto a separation of the two currents has been effected by introducing an air-break in the direct circuit including the tube, with which arrangement the tube is traversed only by the discharge due to the "break" current. The reason of this phenomenon may lie in the different maximum tension of the current on "making" and "breaking" contact, and may correspond to the observation of telegraphists, that discharges of great tension (as observed in electrical storms) will sooner traverse a short distance through air in the form of a spark than traverse a long metallic circuit in the form of a current.

Such phenomena show that in the case of discharge through gases, the division of currents cannot be calculated according to Ohm's law. This we see from the so-called Holtz's "funnel-tubes." If two similar tubes are placed opposite to each other side by side in the same induced current, then at suitable pressure of gas the current does not subdivide itself between the two in any definite ratio to the resistances of the tubes, but the one tube remains entirely dark, the current goes altogether through the other.

The law according to which currents divide when the discharge takes place in gas must therefore be investigated empirically in the first place. I take this opportunity of remarking that I have made an erroneous assumption on this subject in a series of experiments in my book previously referred to, which, however, does not actually affect the result obtained. I believed myself justified in assuming as the evidence of certain phenomena (p. 146), that if a part of the discharge traverse a metallic circuit from the kathode *a* to an electrode *b*, and then the resistance of a moistened thread be introduced between *b* and a wire *c*, that then the

One of the experimental tubes is represented in fig. 1. A cylindrical tube is provided with a flat kathode placed



at right angles to the axis of the tube, and nearly as large as the section of the tube. The anode is placed close in front of it, or in other cases consists of a very short wire in the plane of the kathode itself. Inside the cylinder is a movable partition *c*, consisting of a short glass cylinder terminated by a plane surface at the end turned towards the kathode. In accordance with what I have noted on former occasions*, the positive light disappears for such an arrangement of the electrodes when the exhaustion has reached a certain limit, or it is confined to the immediate neighbourhood of the anode; the kathode-light, on the other hand, expands to any extent if the exhaustion is sufficient, and the expansion of its rays is limited only by their striking upon a solid wall. Hence, when a sufficient exhaustion has been reached, we can vary the expansion of the kathode-rays within wide limits by sliding the piece *c* (by inclining the discharge-tube and tapping it) along the tube; since its length is always equal to the distance between the kathode and the end-surface of the movable cylinder, which can be moved right up to the anode†.

If now the expansion of the kathode-light is made to vary in the ratio 1 : 30, the total resistance of the discharge at low pressures does not alter so much as in the ratio 1 : 1·05. Hence the resistance of the kathode is a vanishing quantity in comparison with the resistance at the surface of the kathode.

Hence we see that the resistance of the whole quantity of gas contained in a discharge-tube becomes smaller the more

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density of the discharge at *a* is not changed. There is in fact a certain change; but its influence upon the phenomena considered in the place referred to is so small that, upon repetition of these experiments with actual constancy of density at *a*, results were obtained partly exactly corresponding, partly nearly corresponding.

The examples given on p. 149 for the magnitude of the deflective power in a particular case consequently represent these values at least very nearly. The same method was employed (p. 131) to confirm a result obtained by two other methods; so that the result given there is not affected by the failure of the experiment in question.

* Goldstein, Phil. Mag. iv. p. 362; 'A new Form of Repulsion,' p. 8.

† In order to resist blows without deformation, the anode in these cases was made of strong iron or steel wire.

the density of the gas is diminished; the space in which the discharge takes place conducts better the less gas it contains; and since this change is always in the same direction while the density is continuously reduced, so far as the experiments can be extended, we must conclude that the greatest conductivity would be obtained after complete removal of the gas. But after complete removal of the gas the discharge-space contains only *free æther*; and I therefore regard this as the true medium of the discharge. Any gas present seems to act only as a hindrance to the æther.

Detailed speculations on the concrete form of the motion of the free æther to which the discharge is due are, in my opinion, premature.

We are not justified in regarding the discharge as essentially a progressive motion of the æther so long as, on the one hand, we regard Doppler's principle as valid in optics, and, on the other hand, refuse to attach any considerable value to the progressive motion of gas-particles in the discharge. We must then ascribe to the motion of the æther amongst the relatively stationary gas-molecules an optical effect precisely similar to that produced by the motion of gas-molecules in æther at rest. Experiments on the constancy of gaseous spectra when observed in directions parallel and at right angles to that of the electric rays, show at once the absence of progressive motion of the æther (of velocity comparable with the velocity of propagation of the ray-discharge), after we have shown by other experiments* the relatively stationary condition of the gas-molecules.

It appears to me safest to characterize the motion of the æther in discharge as *radiant*, in accordance with what has been previously stated†. Every particle concerned in a pencil of negative light assumes the same form of motion which is excited at the point of origin of the pencil.

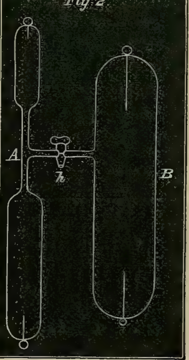
The behaviour of the discharge towards magnetic forces has frequently been brought forward in support of the view that the gas-mass forms the medium of the discharge, since this behaviour may obviously be explained by regarding the gas-molecules as carriers of electricity. If the magnet acts upon the positive light of an equatorially-placed cylindrical tube with electrodes at the end, and finally compresses the light which at first filled the width of the tube into a thin thread lying in the equatorial plane against the wall of the tube, it is generally assumed that the gas-molecules are compressed together with the electricity; I have not found this confirmed.

* Goldstein, 'A New Form of Electric Repulsion,' Chap. IV.

† Phil. Mag. x. p. 184.

A piece of metallic sodium was introduced into a cylindrical vessel 4 centim. in width and more than 20 centim. long, having the electrodes at its ends; and the tube was then quickly filled with dry nitrogen. After the tube had been so far exhausted that its positive light filled the section of the tube, the sodium was brought upon a part of the wall of the cylinder, in a horizontal position, played upon by the positive light. The sodium was next warmed until no more hydrogen was evolved, the tube was refilled with fresh nitrogen, and exhausted again to the same density as before. The sodium is then heated strongly until it begins to volatilize, and the discharge, which was reddish purple before, assumes a golden-yellow colour in its neighbourhood. If the heating be carefully managed, it is seen that the sodium vapour diffuses itself very slowly; so that the discharge in the upper part of the tube still shows the red colour due to the nitrogen, whilst it is of a golden yellow in the lower part of the tube. If the tube be brought in a horizontal and equatorial position near to a strong magnet, whose poles are so placed that the positive light is drawn upwards, the discharge, which at first filled the whole width of the tube, is concentrated into a thread of greater or less tenuity against the upper surface of the tube. But this thread possesses the pure purple colour of the nitrogen discharge without any trace of the sodium-yellow. The sodium vapour is consequently not displaced by the magnet, together with the current, as we are accustomed to see with movable carriers of electricity; the current seems to obey the magnet without affecting the gas-molecules. The result is exactly the same in experiments made with the Holtz machine instead of the induction-coil.

Fig. 2.



I have further examined whether it is possible to recognize a transport of gas-molecules by means of the magnet, in the local increase of density which must result from the assumed transport in a closed space traversed by the discharge. Two discharge-tubes, A and B, fig. 2, were joined together in the manner shown by means of a tube in which a stopcock was inserted. A second stopcock at the end of a short tube shuts B off from the pump during the time occupied by an experiment. The cylindrical portions of A were sufficiently long to show stratified positive light, at least in the cylinder containing the anode,

when pure dry air was employed. The distance between the similar boundaries of the different layers, or the number of layers in a given distance, is a very delicate test for change of density in the gas. Changes in the residual gas which correspond to less than $\frac{1}{100}$ millim. mercury are shown by very marked changes in the interval between the layers*.

The current of a coil was first sent through A, the number of layers in the anode-cylinder determined, and the boundary of each on the kathode-side marked on the tube with ink, thus marking the size of each layer. The current was regulated so as to give perfectly steady stratifications. Opening and closing the tap *h*, before or after turning on the current, did not affect the position of the stratifications. The combination was now brought near to an electromagnet while the current was passing and while the tap *h* was open, the poles of the magnet being so placed that the discharge appeared compressed towards the side of the tube B furthest from the tube A. If this concentration of the discharge depended upon an increase in density of the gas, then the gas in B outside the column along which the discharge passes must be rarefied, and gas from A would enter and rarefaction in A would result. After a few seconds *h* was closed, the current of the magnet was interrupted, the tube removed from the neighbourhood of the magnet to guard against the effect of any residual magnetism, and the discharge again sent through A. The number and position of the layers were found to be exactly the same as before the action of the magnet on the discharge.

The same result was obtained by operating as follows:—The initial density of the stratified discharge of an induction-coil through A was noted; then, after interrupting the current of the coil, the rapidly following discharges of a powerful Holtz machine were sent through B; and this discharge was subjected to the action of the magnet. On now again passing the current of the coil through A, there was no sign of any change in density; any change which had taken place must therefore have been less than $\frac{1}{100}$ millim. mercury; on the other hand, the change of density necessary to produce (when possible) effects upon the discharge, similar to those produced by the action of the magnet at constant density, must be measured by centimetres of mercury. The theory which makes the gas molecules the carriers of the current is therefore in no way supported by experiments with the magnet.

The view which I take of the part played in the dis-

* By the interval between two consecutive layers I understand here and elsewhere the distance between the bounding surfaces turned towards the negative pole.

charge by the free æther is commonly supposed to be refuted (where it is mentioned at all as a possible case, *e. g.* in text-books) by a reference to the experience of spectrum-analysis. If the æther were the vehicle of the discharge, it is said, all gases would give the same spectrum—the spectrum of æther—when subjected to the discharge. But since each gas has a special characteristic spectrum, the gass mass must be regarded as the conductor of the electricity. But it is well known that the æther itself has no power of emitting light. The fact that it has no "spectrum" is therefore no evidence that it cannot conduct electricity. With equal justice it might be argued that the phenomena of light and heat due to a current can only be produced in the molecules of the conducting substance; every conductor in whose mass non-conducting particles are embedded proves the contrary. The luminosity of a gas produced by the discharge depends entirely upon its molecules possessing the form and period of oscillation which are necessary for the emission of visible rays. That this vibrating motion is accompanied by motion due to electricity, executed by the particles themselves or their æther-envelopes, does not seem to be necessary; but, as the phenomena of fluorescence and phosphorescence in sunlight show, the molecules of bodies may execute motions of the form and period of the vibrations of light by taking up the vibrations of the surrounding free æther.

I assume that a similar process take place when a gas is rendered luminous by the electric discharge. The discharge itself represents a motion of the free æther, and is in itself non-luminous. This motion of the æther disappears, being communicated to the gas-molecules and their constituent atoms; the particles of each molecule then vibrate in accordance with their special structure and the conditions as to elasticity of the molecule, and communicate again to the æther the transversal vibrations so produced as such; thus the original motion which the æther possessed as electricity is converted into light, and of course into light whose oscillation-periods depend upon the specific nature of the gas-molecules. The difference in spectrum between chemically different gases thus in no way disproves the conduction of electricity by the æther. I thus regard the luminosity of gases traversed by the electric current as a phenomenon of resonance. I should not be disposed to regard it as a phenomenon of fluorescence or phosphorescence, for two reasons:—(1) Because in fluorescence and phosphorescence the vibrations are transferred from the æther to the atoms, and back again to the æther, without changing their character as transversal

vibrations; here, on the contrary, a motion of the æther which does not consist of transversal vibrations is converted into transversal vibrations. We have, however, a phenomenon analogous to this in acoustic resonance, where we see the longitudinal motions of particles of air transformed into the transverse vibrations of a resounding string. (2) I am disposed to reject the name phosphorescence for the phenomena under consideration, because, according to all the ideas which we have so far associated with the name phosphorescence, an hypothesis as to the temperature-conditions of the discharge would be introduced by the choice of this name, since the temperature of a gas is always supposed to be lower than that of a gas of like emissive power rendered luminous by heat. But this, even assuming that the conclusions of E. Wiedemann as to the temperature of the discharge should be confirmed by further experiments, is not yet accepted as the true character of the light of the discharge; and my speculations on the nature of the discharge do not in any way prejudice the question of the temperature of the discharge. For the present I leave this entirely out of the discussion.

The assumption that a vacuum conducts electricity has consequences which are far-reaching, especially in the domain of cosmical physics. The usual fate of attempts to found a cosmo-physical theory upon experimental results is scarcely such as to encourage imitators. At the same time I venture to point out at least so much as this, that certain terrestrial phenomena of an electric or magnetic nature which, because of the coincidence of their periods or epochs with solar changes, have been explained as due to the statical influence, magnetic induction, &c. of the sun's mass, might possibly be more conveniently referred to electric currents radiating through interplanetary space from the central body. Experiment shows no limit to the expansion of that remarkable motion which we observe in the kathode-rays as we eliminate the ponderable medium more and more completely—it is conceivable that the sun radiates electric rays as well as light-rays through space. We see that even when the two poles are placed close together, the kathode-rays stream out into space without limit, without reference to the position of the anode; consequently for electrical communication with the sun, it would not be necessary that the earth should be the source of electricity or pole of the current, but discharges for which both poles are situated on the sun might produce rays radiating from the sun into space.

II.

I consider two processes to be necessary for the production of the discharge:—(1) A change in the condition of the æther, preceding the discharge, which produces a certain condition of unstable equilibrium in the arrangement of its parts: this condition may be called, for shortness, tension of the æther. (2) The restoration of equilibrium: this constitutes the discharge itself.

The tension which precedes the discharge is not equally great in all cross sections of a discharge-tube, even when the tube is of equal section throughout; it may even equal zero in certain parts of the tube. The tension has either finite or maximum values at the surface of the metallic poles and at those points which appear as points of issue of the separate positive layers or of the secondary negative pencils. The resultant of the opposing force produced by the tension on each element of the kathode is directed away from it; at the other points of issue also, it is directed at each point towards the side turned away from the kathode. When the restoration of equilibrium commences, a motion results in consequence of the finite or maximal tension on the surfaces, which advances to the side of each surface of issue remote from the kathode, and, originally excited in free æther, transforms itself secondarily on its way into transversal vibrations of the material atoms. The distances in which the tension before the discharge was zero, and in which the motion excited at the surfaces of issue does not propagate itself, remain dark; such places are the distances between the positive light, on the one hand, and the kathode-light or secondary negative light, on the other hand.

The greater the exhaustion becomes, the more do the distances between the surfaces of origin increase, and at the same time also the distances to which the motion excited at the surfaces of issue extend. This latter increase, however, is completed more rapidly than the increase in the distances between the surfaces of origin; hence it comes that one and the same section of the space occupied by the discharge may be affected by motions which radiate from two or more surfaces of origin. (Penetration of the kathode-light into the positive light or of the stratified pencils into each other.) Experience shows that such motions penetrating each other do not sensibly alter each other when their directions are the same; but when their original directions are inclined to each other at a considerable angle, they show marked phenomena of deviation *.

* Goldstein, *Wien. Ber.* 1876, 23 Nov.

That the so-called æther-envelopes of the gas-molecules or atoms take part in the emission of light resulting from the discharge is a matter of course; but the part which they play in the processes of charging and discharging must remain for future discussion. The forces exerted by the material particles upon which the formation of the æther-envelopes depends, tend to produce a different disposition of the æther from that which would result from the electrical forces only. Consequently the more gas-molecules are included in the space occupied by the discharge, the greater the electrical forces must be in order to bring about the disposition of the æther which must precede the discharge. Hence we understand how the gas acts as a hindrance to the discharge, and why conductivity of the space occupied by the discharge continually improves as the gas is more and more completely removed. In any case, I am unable to accept E. Wiedemann's view *, according to which the æther-envelopes are the real medium of the discharge. If, moreover, the æther-envelopes suffer deformations without the free æther taking any part in the process (and in the case of the kathode-discharge Wiedemann excludes any such participation), then, as regards the velocity of propagation of the discharge, Wiedemann must assume a pure distance-action between the æther-envelopes, since in highly rarefied gases we regard the times during which the æther-envelope and the sphere of action of a molecule are in contact with those of other molecules, or penetrate them, as small in comparison with the times during which the sphere of activity is isolated.

The assumption that the direction of the negative current from the kathode is the direction in which the electric discharge is propagated in the kathode-light, and also in the secondary negative pencils and positive stratifications, contrary to the usual view, seems to be justified by numerous experimental results. I would call attention, first of all, to the phenomena of shadows, which, formerly observed only with the kathode-light, have caused this phenomenon to be represented as a motion from the kathode, even by the defenders of the convection theory.

If a solid body be placed in a pencil of the kathode-light, or of the secondary negative light, then, as may be observed directly, that portion of the pencil falling upon the object which lies between its end turned towards the kathode and the object itself remains in every case intact, but that portion of the incident pencil lying on the further side of the space occupied by the object is wanting. The shadows previously described †,

* E. Wiedemann, Wied. *Ann.* x. p. 245, 1880; Phil. Mag. [5] x. p. 419.
† Phil. Mag. [5] x. p. 236.

formed in the phosphorescent surfaces excited by the positive light, and their position indicate similar behaviour. If the electric rays in the kathode-light proceeded from its exterior boundary towards the kathode, and in the secondary negative and positive pencils from the side of the anode towards the side of the kathode, then, on introducing an object, the pencil of rays would, on the contrary, remain intact from the exterior boundary up to the object, and the shadows would appear upon the wall upon the kathode side of the object.

Another argument for propagation in the direction of the negative current is found in the phenomenon described above, that the properties of secondary negative rays are, even for considerable distances, such as correspond to the conditions which exist at the negative boundary of the pencil of rays—that is, the one nearest to the kathode. The pencil, which with increasing evacuation radiates continually more and more from the mouth of a narrow tube opening into a wider vessel, contains rays possessing the properties of the light of narrow tubes. If the pencil had its origin in the wider vessel and propagated itself from it into the narrower tube, we should expect to find its properties more in accordance with the conditions of discharge offered by wide tubes. The pencil between a narrow cylinder and a wider one following upon this upon the side of the anode would show then the same colour and spectrum as those pencils which have their origin in the wide cylinder and compose the column of its positive stratifications.

A further criterion for the direction in which the electric rays propagate themselves is found in their magnetic behaviour as described above for the kathode-rays, in accordance with Hittorf's conclusions.

It is characteristic of this behaviour that if a (sufficiently weak) magnet is allowed to act upon the end of a long kathode-pencil remote from the kathode, only this end is affected by the magnet, whilst those portions of the pencil near the kathode retain their form and position unaltered. If the magnet is brought into the neighbourhood of the kathode itself so as to act upon the portions of the rays nearest the kathode, then the whole pencil is deflected together with these portions even to its furthest point, upon which, in consequence of its great distance, the magnet could exert no action directly.

The electric particles (or the electric motion) at the end of the ray remote from the kathode therefore follows the direction impressed upon the particles at the kathode itself; but the particles at the kathode are not influenced by action upon the particles at the further end. Both phenomena agree with

the theory that the particles at the outer end of the ray were previously at the kathode, and are immediately opposed to the view that the particles nearest to the kathode have already passed through the place occupied by the remote ends; that is, the motion in the kathode-light must propagate itself from the kathode outwards.

Exactly correspondent are the phenomena of the rays of the secondary negative pencils, and also of the rays of the separate positive stratifications, when these are sufficiently expanded by high exhaustion. Hence the discharge propagates itself also in each separate stratification from the bounding surface on the kathode side to the boundary on the side of the anode. The often-mentioned phenomena of deflection are to be interpreted in a similar manner. If K (fig. 3) be the projection of a plane kathode, K′ that of a thin wire, *s* the natural direction of an electric ray issuing from K, then the ray through K′ takes the form *s* K′ *s*′; at K′ it bends round through a considerable angle, and beyond K′ follows again a straight course, which, however, deviates considerably from the direction *s* K′; *i. e.* the portion of the ray beyond K′ obeys the deviation which was exerted upon the electrical particles at K′. Hence the forces which produce motion at any point of the ray influence also all portions of the ray beyond this point, but are without influence upon the portions between the first point and the kathode. This is very simply explained by the hypothesis that the electrical motion in the ray propagates itself from the kathode outwards (in the direction of the arrow).

III.

The velocity and direction of the discharge of a pencil of electrical rays is to be distinguished, *à priori*, from the velocity and direction with which the tension that precedes the discharge propagates itself. We are not here further concerned with the velocity of this tension, but only with its direction. I believe that the phenomena observed indicate very plainly a propagation of the tension also in the direction of the negative current; that is, the tensions for the separate positive stratifications are developed in the same order of time as that in which they follow each other in space from the kathode towards the anode. I draw this conclusion from the fact that the position and peculiarities of the separate completely formed stratifications, and in particular the position of

the heads of the stratifications, *i. e.* of the points from which the separate discharges formed by the stratifications issue, depend altogether upon the position and peculiarities of the kathode, and not at all upon the conditions of the anode*.

Let the discharges pass in a cylinder with terminal electrodes which can be moved along the axis of the vessel towards each other by means of an arrangement which need not at present be further described.

If the anode in such a vessel is caused to approach the fixed kathode, no displacement whatever of the stratifications in front of the anode is observed; they remain altogether immovable and unchanged, so far at least as their continued existence is consistent with the new position of the anode. Since the positive light in every case reaches only to the anode, the layers which were situated in the portion of the tube

* The experiments described in what follows are performed with stratified discharges in pure dry air or in dry highly exhausted hydrogen, produced from a sufficiently powerful induction-current by regular interruption of the primary current. Under these conditions we obtain thick stratifications which do not vibrate to and fro like the so-called "saucer" stratifications, but, with a constant pressure of gas, preserve their position unaltered. I think it necessary to mention this, because to many who are still accustomed, upon mention of stratifications, to figure to themselves only the "saucer" stratifications and their behaviour, the mention in the following of motionless stratifications with constant intervals between them may appear surprising. The thick stratifications, strange to say, are always treated, sometimes as quasi-pathological developments, as phenomena which are disturbances of the normal phenomena of stratification; sometimes as optical illusions, caused by rapid vibration to and fro of the "saucer" stratifications assumed to exist alone. According to the evidence I have given, the meaning and mutual relation of the different forms of stratification are tolerably clear; each separate layer in a cylinder is qualitatively analogous to the discharge at a kathode which occupies the whole section of the cylinder. When the exhaustion is small this kathode-light is only a thin layer, corresponding exactly to the thin "saucer" stratification. If the exhaustion proceeds, the electric rays which make up either structure lengthen and so increase its thickness; and just as the kathode-rays finally lengthen so much as to completely fill the dark space and reach to the first positive layer, so the rays of stratifications extend so much as to completely occupy the dark spaces between them. As the density of the gas decreases the thickening proceeds further, the intervals between the heads of the layers continually increasing, and the rays issuing from the heads continually occupying the increasing intervals. When the stratifications appear most plainly in a cylinder with dry air, their thickness is very nearly equal to the diameter of the cylinder, so that with wide vessels they are of considerable thickness. As little as a kathode-discharge with extended rays is a monstrosity or disturbing phenomenon, or consists entirely of a luminous layer vibrating to and fro, so little can we entertain similar views about the thick layers; and as the laws of the kathode-light may be studied most easily in the longest pencils of it which can be obtained, so also it is just the thickest layers which represent the phenomenon most completely developed, which are in the first instance best suited for the study of the stratification.

passed over by the anode in its approach to the kathode disappear one after the other, presenting the appearance of the layers being gradually absorbed by the anode. If the anode is caused to move from its original position away from the kathode, then all the layers originally present remain unaltered, whilst new layers appear in the space left by the anode, of which each immediately upon its formation remains completely indifferent to the further motion of the anode.

Let us now move the kathode, and first let the motion be an approach to the fixed anode; at once the whole of the layers present begin to move, and are displaced each in the same direction and exactly by the same amount as the kathode itself. (In the distance occupied by the discharge, shortened by the approach of the poles, there is consequently room only for fewer layers than before; each layer disappears as soon as it is pushed up against the anode by the motion of the kathode.) If the kathode is removed from any initial position whatever further away from the anode, all the layers present follow the kathode, and keep exact time with the motion of the kathode itself; new layers appear in the space between the last of the layers originally present and the anode as the kathode moves further away, and each immediately after its formation follows the motion of the kathode*.

The interval between every two layers in a cylinder is so little different in passing from one pair to another, that in a cylindrical column of stratified light at given density of gas and intensity of discharge, we may speak simply of the stratification-interval of the column.

The number of layers present is therefore equal to the quotient of the length of the column by the stratification-interval. If the distance of the electrodes varies continuously, this quotient will only be a whole number in particular cases. If the division of the space occupied by the discharge into layers advanced from the anode, then in the case when the length of the positive light is not divisible by the stratification-interval without remainder we should expect that the incomplete and abbreviated layer corresponding to this remainder would be found at the negative end of the positive light, whilst at the anode there would be nothing but complete layers.

Observation shows exactly the contrary: the positive layer nearest the negative end of the positive light, *i. e.* nearest to the kathode, preserves the same constant extension with every distance of electrodes; so also the following layers, only the one directly in contact with the anode shortens or lengthens

* Goldstein, Phil. Mag. [5] iv. p. 362.

exactly in proportion as the excess of the above-mentioned quotient above a whole number changes.

The changes of colour are also strikingly characteristic of the influence of the kathode, as explained above. The consecutive layers of a column of positive light may show very striking differences of colour, even when no differences in form and magnitude can be perceived; these differences are very marked when hydrogen is employed.

The colour of a layer when the difference of electrodes changes is always, *cœteris paribus*, dependent on its position with reference to the kathode. Suppose, for example, that we observe a cylinder the positive light of which is divisible into five layers, the one next the kathode being blue, and the following ones in order being yellow, red, greyish red, and grey. Next to the anode we have, therefore, a grey layer. If the distance of the electrodes is now diminished by the length of one layer, whether by the motion of the anode or of the kathode, it is the grey layer which disappears, and we have a greyish-red layer next the anode followed by the rose-coloured, yellow, and blue layers in order. If the distance between the electrodes be further diminished by the length of one layer, the greyish-red layer disappears, and the rose-coloured layer is in contact with the anode, followed by the yellow and blue layers in order. When the poles are caused to approach further so as to leave only two layers, the yellow layer is next the anode, and the blue layer follows it. If, therefore, we count the layers from the anode, then with every change of distance between electrodes, the first, second, and every layer change colour; but if we count from the kathode, then the colour of the nth layer is independent of the distance of the electrodes, and each layer present possesses always the same colour.

Hence the colour of each layer is regulated by the position of the kathode, and depends on its position in the series, counting from the anode.

Lastly, we may vary the size of the anode indefinitely without causing any change in the position of the layers present; but if the magnitude of the kathode be changed, the position of all the positive layers changes. The smaller the kathode becomes, under conditions otherwise similar, the larger becomes the interval between the kathode and the first positive layer, but the interval between the positive layers is not altered; so that each single layer lies further from the kathode the smaller the kathode is made.

We cannot, however, assume that the kathode, or the physical conditions which obtain at the kathode determine the conditions of tension and discharge of the whole stratified

column; but it appears that the position and properties of each separate layer depend mainly or entirely upon the position and properties of the layer preceding it on the side of the kathode. The influence of the kathode on all the members of a stratified column, which appears so markedly in the experiments just described, would then be only an indirect one, inasmuch as the properties of the kathode determine the properties of the kathode-light; this determines the position and properties of the first positive layer, this the position and properties of the second layer, and so on. This conclusion is drawn from experiments on the secondary negative light.

We saw that in a cylinder where the kathode is moved, all the layers move in the same direction as the kathode and through an equal distance. If now we introduce into the cylindrical tube C (fig. 4), with movable kathode K, a por-

Fig. 4.

Fig. 5.

tion of tube R fitting C closely and also movable in it, having a narrow opening at x, then, as already explained, x acts as a secondary negative pole for the portions of the whole discharge between x and the anode A*.

If now, whilst R retains the same position in any portion of the discharge-cylinder, the kathode K is moved, all the layers between K and x move as in the previous experiments; but the layers between x and A remain immovable, in spite of the displacement of the kathode.

If, on the other hand, K be fixed and R be displaced, so that the secondary negative pole x moves with the secondary negative light radiating from it, then all the layers between x and A are displaced exactly like the stratifications of a simple cylinder having a metallic kathode at x.

If the separate layers of the discharge show different colours, then we observe further that, when K moves, the colours of all the layers between K and x behave as previously described

* A may with advantage be made short, and placed somewhat excentrically but parallel to the axis of the tube, so that it may not be struck by the movable piece R.

for the simple cylinder, but the colours of the layers between x and A show no regular relationship to their position in order from K; their colours remain the same however their position in order varies. But if x be moved, the same law holds good for these layers as if x were a metallic kathode—the colour of each depending on its position in order from x, and the colour of every nth layer, counting from x, remaining the same for every position of x.

The dependence of the stratifications upon their secondary negative pole, and the complete analogy with the dependence upon the metallic kathode, is seen, lastly, also in the influence of the magnitude of the secondary pole. If its magnitude be diminished the layers become further apart, as if from a diminished metallic kathode; and the displacement is in both cases more marked when the surface of the pole is diminished in a greater ratio*.

If we have now a tube (fig. 5) provided with two secondary poles x and y of this kind, of which only x is movable and y is fixed (the piece R′ of which y is the mouth may be conveniently united with the wall of the large vessel by fusion when

Fig. 6

* The diminution of a secondary negative pole may be effected in a variety of ways. Fig. 6 shows in one diagram three different simple arrangements for effecting this:—

In cylinder I. a glass diaphragm is arranged perforated with two round openings of different sizes; a glass ball is also enclosed in the tube, whose diameter exceeds that of the largest opening. By allowing the glass ball to rest upon the one or the other of the two openings, the discharge issues from a larger or smaller secondary pole. (Of course the opening acts on the side towards the kathode as a secondary positive pole.)

In cylinder II. the opening x of the communicating glass tube is the secondary negative pole; a glass rod, provided at one end with a knob to prevent its falling completely through, is movable to and fro in r. It is clear that by this means the magnitude of the opening x may be varied.

Cylinder III. shows the simplest arrangement, a glass tap the perforation of which replaces the communicating tube. The magnitude of the secondary pole is a maximum when the tap is completely open. If we gradually turn the tap from this position, and so gradually reduce the magnitude of the secondary negative pole, we see the layers gradually recede from the pole. The advantage of this arrangement is found in the power of gradually altering the magnitude of the pole; its disadvantage in the gradual alteration of the quantity of gas contained in the tube, by the evolution of gases produced by the action of the discharge on the substance with which the tap is lubricated.

the apparatus is constructed), x lies between the kathode K and y. If the kathode K is movable, we observe first that its motions only affect the stratifications between it and x, but, on the other hand, all between x and y, as well as those between y and A, are unaffected. If x is moved, we find that the motion of this pole causes a motion only of the stratifications between x and y; the stratifications between y and A remain unmoved. In the same way, when the magnitude of the negative pole varies, it is found that only changes in the pole y affect the position of the stratifications between y and A.

Hence the position of each stratification depends on the position and properties of the secondary negative pole, or pencil of secondary negative light nearest to it. But since each separate positive layer, even in a simple cylinder, is, as I have shown*, only a form of the secondary negative pencil (the section of its origin is itself a secondary negative pole), it follows that the position and properties of each separate layer, even in a simple cylinder, do not depend so much upon the kathode and kathode-light as upon the position and properties of the layer immediately near it. If, then, in a simple cylinder all the stratifications are put into motion by displacing the kathode, it follows that the motion of the kathode itself properly causes only a corresponding change in the position of the kathode-light which issues from it; the displacement of the point of origin of this last displaces the surface of origin of the first positive layer; this change displaces the surface of tension for the second layer, and so on.

I have mentioned that, in a simple cylinder, the consecutive intervals between the stratified layers of the positive light differ very little from each other; but if we observe also the very small differences which present themselves, we find that the intervals gradually decrease from the kathode towards the anode. If the space in which the discharge takes place be contracted at any point whatever so as to produce a secondary negative pole, then the intervals diminish only up to this pole; beyond it the intervals suddenly increase and a new series of decreasing intervals begins, again increasing beyond a new secondary pole, and so on. We find, then, that the magnitude of each stratification-interval is determined by the ratio of the secondary negative pencil produced by the change of section which immediately precedes the interval under consideration.

In passing to infinitely small changes in section when the secondary negative pencil passes into a positive layer, we find that the magnitude of the interval between any two layers

* Goldstein, *Berl. Monatsber.* 1876, p. 280: Phil. Mag. [5] iv. p. 361.

depends on the properties of the layer forming the component of the pair nearest to the kathode; or, shortly, each layer, or the conditions existing at its point of origin, always influence the layer following next to it, on the side of the anode, but does not influence the preceding, on the side of the kathode. The conditions under which the nth layer forms seem to stand to the properties of the $n+1$th layer in the relation of cause and effect; and hence it seems to me only a verbally different expression of the observed facts if we assume, as above, that the propagation of the electrical tensions, or the production of the separate layers, takes place in the direction of increasing values of n, *i. e.* advances from the kathode towards the anode.

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XLII. *Carbon Dioxide as a Constituent of the Atmosphere. By* Ernest H. Cook, *B.Sc.* (*Lond.*), *A.R.C.S., Lecturer upon Chemistry and Physics at the Bristol Mining School**.

OF all the agents which have brought about geologic changes and modified the surface of the earth from time to time, the atmosphere seems to have been the least studied. Nor is this very surprising when we remember the peculiarity of its action. So general and cosmopolitan are its effects that their very abundance causes us to overlook them—and, again, so slowly acting that the changes effected require the employment of long periods of time. The two constituents of the atmosphere which have been most active in producing these changes are the oxygen and the carbon dioxide. The latter substance occurs in the air in such a relatively small amount that we are apt to underrate its influence. But when it is remembered that, were it not for the presence of this substance in the air, no coal and very little limestone could have been formed, we at once see its importance. In fact, to come somewhat nearer home, without carbon dioxide in air no vegetable growth could take place; and without plant life very little, if any, animal life would occur. Thus this substance, although in itself inimical to most forms of animal life, is absolutely necessary in the atmosphere in order that those animals may exist. In the present paper an attempt is made to consider some of the results arising from the presence of this substance.

* Read before the American Association for the Advancement of Science at Montreal, on August 25, 1882. Communicated by the Author.

Amount of Carbon Dioxide in the Atmosphere.

This question has been made the subject of experiment by many of our leading chemists. In order to calculate the absolute amount, we require to know two things—viz. the capacity or weight of the air, and the percentage of CO_2 which it contains. Fortunately the data for doing this have been determined with very great accuracy. The lengths of the diameters of the earth have been determined to be very nearly 7899 miles for the polar and 7925½ for the equatorial ; "and in these measures it is pretty certain that there is not an error of a quarter of a mile" *. Applying the ordinary rule for the cubic content of an oblate spheroid, we obtain 259,026,554,299 cubic miles as the capacity of the earth. Now the height of the homogeneous atmosphere is found to be 26,214 feet †, or very nearly 5 miles ; calculating the capacity of the spheroid formed by adding this distance to the lengths of the diameters given above and subtracting the capacity of the earth, we obtain the cubic content of the atmosphere supposed homogeneous: this is found to be 591,647,337 cubic miles. With regard to the amount of carbon dioxide present in the air, the older experimenters, Dumas and Boussingault (*Ann. Ch. Phys.* iii. pp. 257, 288), Löwy and Saussure (Pogg. *Ann.* xix. p. 391), have published results which yield a mean of 4 vols. in 10,000 of air, or 4½ parts in 10,000 by weight. Thorpe (Journ. Chem. Soc. vol. xx. p. 189) has shown that over the sea the average is 3 vols. in 10,000 ; while Saussure states that at high altitudes the proportion of dioxide is greater than at lower levels. Without deviating very far from the truth we may take 4 vols. in 10,000 of air; and we thus find (assuming capacity of air to be 592,000,000 cubic miles) 236,800 cubic miles as the amount of CO_2 in the atmosphere. Finally, calculating from the specific gravity, we find the weight to be 4287 billions of pounds. Expressed on the metric system these figures become:—

Cubic capacity of air... 2,439,987,200,000,000,000 kilolitres.
Weight of CO_2 in air... 1,913,685,908,480,000 kilogrammes.

I have given these calculations somewhat in detail because of the great difference between my numbers and those hitherto published. Thus, Dumas and Boussingault (*op. cit.*) say that the air is equal in weight to 581,000 cubes of copper each having a side of 1 kilometre: this gives 4,200,000,000,000,000,000 kilolitres as the capacity of the air, or very nearly 40 per cent.

* Herschell, 'Familiar Lectures,' p. 53.
† Maxwell, 'Theory of Heat,' p. 228.

too high. Again, Roscoe and Schorlemmer ('Chemistry,' vol. i. p. 449) state that "the amount of CO_2 in the atmosphere reaches to upwards of 3000 billions of kilogrammes," which is about 33 per cent. in excess of the truth*.

Sources whence the Air derives its Carbon Dioxide.

These are mostly natural; but the progress of civilization has added a large artificial supply to those already existing. We may state them as follows:—

(1) Combustion of carbonaceous bodies.
(2) Respiration of animals.
(3) Decomposition of vegetable and animal substances.
(4) Volcanos and other subterranean supplies.

Under the first heading is included the amount produced by the burning of coal, wood, peat, &c. From the most recently issued statistics with regard to the amount of coal raised in the world that I have been able to consult†, I find that for the last three years at least 280 millions of tons have been raised annually. This is probably a slight underestimate. Assuming that 75 per cent. of this consists of pure carbon, which if completely burnt in air would produce CO_2, and allowing a further 10 per cent. for the carbon thrown away with the ash, we leave 182 millions of tons which are annually converted into carbon dioxide. This will produce 1,800,000 tons per day, or very nearly 1800 millions of kilogrammes per day. Assuming that by the combustion of wood, peat, oil, &c. there is added one third more, we produce a total of 2400 millions of kilogrammes daily.

In the case of the respiration of animals we can only form an approximate estimate. The population of the world is at present about 1500 millions; and it has been shown by experiment that each individual produces on an average about a kilogramme of CO_2 per day of 24 hours. Thus the human race, in respiring, add to the air about 1500 millions of kilogrammes of carbon dioxide per day. Remembering the large

* The above calculations are made on the figures deduced from the results of the experimenters cited above. Recent investigations have, however, thrown some doubt on the correctness of these numbers, the general opinion being that 4 vols. in 10,000 is much too high. Thus, Fittbogen and Hasselbarth (*Chem. Centr.* 1875, p. 694) give 3·4 vols. in 10,000 as the average; Farsky (*Chem. Centr.* 1877, p. 198) found 3·4, while more recently Reiset (*Comptes Rendus*, lxxxviii. pp. 1007–1011) deduces 2·942. Taking the mean of these numbers, we have

Weight of CO_2 in air .... 1545 billions of kilogrammes nearly.

† Mineral Statistics for Great Britain for 1881; and Smyth's 'Coal and Coal-Mining,' latest edition.

amount of animal life existing on the globe, and also that many of the larger species produce a greater quantity in a given time, we may with a sufficiently near approach to accuracy say that from the lower animals the air receives twice as much daily as from man. Hence from the whole animal kingdom we derive about 4500 millions of kilogrammes.

The amount of dioxide which the atmosphere receives from decaying animal and vegetable substances is impossible to estimate. Most of it is produced in regions far away from the abode of man. That a considerable quantity is produced from this source, however, is evident when we consider the vast quantity of vegetable matter which year after year falls to the ground and undergoes decomposition. In fact, if the estimate of the amount of action exerted by plants given later on in this paper is a correct one, we must conclude that a much greater amount of dioxide is produced by this process than has been hitherto supposed. Although it is evidently impossible to give figures, yet, in order to arrive at a numerical estimate, we may assume that the same quantity is yielded as by man, viz. 1500 millions of kilogrammes daily.

The last source whence the air receives its supply of carbon is from volcanos and the fumaroles and rents in the ground in volcanic districts. The amount thus supplied is enormous, both active and extinct volcanos joining in increasing the quantity. Considering the area occupied by the volcanic districts, and the immense quantities of gas which are given off from the craters and fumaroles, we must readily come to the conclusion that from this source by far the greater part of the atmospheric carbon dioxide is derived. In fact Poggendorff has calculated that at least ten times as much is derived from this source as from all others put together. The numbers given above for the amount yielded by other sources are probably greater than similar numbers deduced by Poggendorff, since the amount of coal used and the population have both increased since his time. Instead, therefore, of taking ten times, if we take five we shall perhaps approach very near to the absolute amount given by Poggendorff. This will give us about 40,000 million kilogrammes daily given to the atmosphere from subterranean sources*.

Taking the whole of these results together, we have that from all sources there is daily added to the atmosphere the

* Supposing this CO_2 produced according to the equation

$$CaCO_3 = CaO + CO_2,$$

we shall have daily decomposed about 90,000 million kilogrammes of limestone.

enormous amount of at least 50,000 millions of kilogrammes of carbon dioxide. Dividing the absolute amount given above by this number, we find *that the amount of carbon dioxide in the atmosphere would be double what it is at present in about one hundred years if there were no means of compensation.* In arriving at this estimate no account has been taken of the amount of oxygen used up in producing the dioxide. This obviously affects the first three sources only; but by taking it into account we should reduce the time somewhat; but practically this correction is so slight that it can be neglected. Poggendorff made a similar calculation, and gave 386 years as the period which it would take to double the amount of the dioxide, supposing there were no compensating influences at work. The discrepancy in the two numbers is explained, first, by the absolute amount of CO_2 in the air being much less according to my calculations than that previously supposed, and also by the circumstance that Poggendorff's estimate of the amount yielded by the combustion of carbonaceous substances was much less, owing to the defective data at his command.

Compensating Influences.

Having now arrived at an estimate of the amount of carbon dioxide daily added to the atmosphere, let us examine the causes which bring about its decomposition and removal from the air. The known causes which are at work producing this change may be considered under three heads, viz.:—

(1) Fixation of carbon by growing plants.
(2) Removal of dioxide by zoophytes.
(3) Absorption of dioxide by inorganic chemical actions.

The first cause here mentioned is one which is essential to almost all forms of vegetable growth. In estimating its magnitude we are met by the want of reliable experimental data, making it almost impossible to arrive at any definite conclusion. It is, however, the only one which restores the oxygen to the atmosphere, in the other two actions the dioxide being absorbed bodily without being decomposed. Also most, if not all, the decomposition effected by plants will occur during the spring and summer, the most active period of plant-growth. The second and third causes act continuously. Certain experiments have shown that a square metre of leaf will decompose in sunlight about a litre of CO_2. Also Mr. Trelawny Saunders, some years ago, calculated for Sir Charles Lyell the area of the land-surface of the globe. The figures he gives are*:—

Total area of land	57,600,000 square miles.
Area of Arctic and Antarctic land .	8,200,000 ,, ,,

* Ansted's 'Physical Geography,' p. xxxviii.

Thus the land-surface bearing vegetation capable of decomposing carbon dioxide amounts to 49,400,000 square miles. A large portion of this land, however, is uncovered by vegetation: cities are built on it; barren mountains rise out of it; and large rivers run through it. Estimating the absolute area of leaf (*i. e.* chlorophyll-bearing organs) borne by the plant-bearing land of the earth as 50 per cent. of the total area, we find that 24,700,000 square miles of leaf are engaged in purifying the atmosphere. This is equal to about 63,973,000,000,000 square metres, which gives the number of litres of CO_2 decomposed per hour. But sunlight only lasts, on an average, about ten hours a day; consequently the total amount daily decomposed is equal to ten times this amount. Finally, allowing 25 per cent. for the diminution of the action which takes place in winter, we find that the enormous amount of 479,000 millions of kilolitres, or over 900,000 millions of kilogrammes of carbon dioxide are decomposed daily. This amount is much greater than that produced from all sources taken together. But it must be remembered that a large portion of the carbon thus withdrawn by plants during the spring and summer months is returned to the air again by the decomposition of the leaf in autumn. Although we have allowed for this above, yet if plant-action is anything like so powerful as these calculations show, that allowance will have to be considerably increased. Again, a reduction, and perhaps a considerable one, will have to be made on account of the respiration which has been proved to take place in some plants during the hours of darkness; but I am unable to find an account of any experiments upon this point. The magnitude of this action given by these calculations is astonishing. This paper was commenced under the idea that the action usually attributed to plants was greatly overestimated, and that their purifying effect was exaggerated. It will be seen that the vegetable life on the globe is sufficient of itself to keep up the purity of the air. The author wishes this statement to be received with caution, because of the unsatisfactory nature of the fundamental experiment upon which the calculations are based, and also of our total want of knowledge of the amount of plant-respiration. This latter action may be much greater than is usually supposed.

The second great action going on in nature is effected by the interposition of animal life. It consists in the removal from sea-water of the carbon dioxide held by it in solution by certain low forms of animal life. The most important of these are Actinozoa and Foraminifera—the former being concerned in the building of coral reefs, and the latter in forming those

immense masses of rock-material of which the chalk and nummulitic limestone maybe taken as examples. Certain other forms of animal life, such as Brachiopoda &c., also add their influence to that of these lower forms; but their effects, however, are comparatively insignificant. The immense influence exerted by these minute creatures is evident when we remember the vast masses of limestone entirely of organic origin occurring in geological formations of all ages. Millions of tons of limestone formed in this way occur in the solid crust of the earth; and every ton of limestone contains about nine hundredweight of dioxide. Nor is this action confined to the past. It is as active now, in all probability, as when engaged in building up those immense deposits of white chalk so abundant in some parts of Europe. Recent deep-sea soundings have revealed the fact that Foraminiferal life still flourishes in the depths of the ocean, while the coral-polypes are still building reefs in the warmer seas. On the other hand, we must not forget that Darwin has shown that these coral-polypes can only exist in water of a certain temperature, which is only attained in the warmer seas, and at a certain depth below the surface of this water. Their influence, therefore, is limited and confined to a comparatively small area of the globe. Another circumstance which seems to have been overlooked by most writers upon the subject is, that this dioxide fixed in the solid state in this way is contained in the water, and not in the atmosphere. It is generally supposed that all of it has been derived from the air; but a very large portion *must have been obtained from submarine volcanic eruptions*, and never formed part of the atmosphere at all. Taking all things into consideration, this cause, although very powerful, seems rather to be one whose influence is only felt after the lapse of many years, and, for activity, cannot be equal to the first one.

The third action going on in nature effecting the purification of the air is a strictly inorganic one. Included under this head are such processes as the conversion of felspar into kaolin, the decomposition of such silicates as hornblende, pyroxene, &c. The large deposits of kaolin and decomposed felspar which are met with in the earth sufficiently prove the magnitude of this action. Calculations were made many years ago by Ebelmen (see the *Receuil des Trav. Scient. de M. Ebelmen*, Paris, 1855), and have recently been recalculated and very clearly stated in an excellent paper by Dr. T. Sterry Hunt, F.R.S.* A glance at the numbers given in these memoirs will show the vast and important effect which these processes must have exerted.

* "Chemical and Geological Relations of the Atmosphere," American Journal of Science, May 1880.

Thus, Dr. Hunt says "that a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 metres, an amount which evidently represents but a small proportion of the results of felspathic decay in the sedimentary strata of the globe." Evidently, then, here we have a cause which has removed, and is removing, a vast amount of carbon dioxide from the atmosphere. Any estimate of the rate of its action is obviously impossible. It must not be forgotten, however, that subaerial felspathic decay is a very slow process, and that therefore the large deposits of decomposed felspar found in the earth seem to point rather to a comparatively slow process acting through an immense number of years than to a rapid process such as that effected by plants.

General Conclusions.

It is of course evident that, if the compensating influences are just equal in amount and in rate of action to the producing ones, the amount of carbon dioxide in the air will remain constant. Unfortunately an insufficiency of reliable data prevents a definite answer being given to such a question. The foregoing considerations, however, seem to show that in all probability the causes at work removing atmospheric dioxide are more powerful than those producing it. As a consequence, the atmosphere is being robbed of this constituent, the greater part of which is becoming fixed in the solid earth as carbonate of lime. But this process has already gone on for so long a time, that there is already fixed in this way an immense quantity of CO_2 equal to many hundreds of times the amount contained in the existing atmosphere. The question of the source of this large amount naturally arises; but the answer to be given must simply be an admission of our want of knowledge. The idea that it all at one time formed part of the atmosphere of the globe has been suggested by Brongniart; and Dr. Sterry Hunt considers (*loc. cit.*) that a universal atmosphere of the same quality as that of the earth exists, from which the carbon dioxide now fixed in the earth's crust has been derived.

There can be no doubt that, unless we accept the latter of these theories, there must at some antecedent period have been an atmosphere covering the globe much richer in this gas than the present one; but whether such an atmosphere would account for the luxuriant vegetation of the Coal Period is at present an open question. If Dr. Hunt's hypothesis be a correct one, it is interesting to remember that the carbon which we contain in our bodies may have existed at one time as a

portion of the body of an inhabitant of the most distant member of the universe. But whichever way we consider the subject in the light of the facts which we have stated, it is full of unusual difficulty, and is singularly devoid of accurate experimental data.

XLIII. *On the Dimensions of the Magnetic Pole in Electrostatic Measure.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE had the honour of reading a letter upon the dimensions of the magnetic pole in electrostatic measure which Dr. Lodge addresses to you this month. His suggestion seems to me to reconcile the views of Prof. Clausius and Mr. J. J. Thomson on this subject. A model of the magnetic system must be made in a substance of the same magnetic permeability as the medium that is to surround the current-system, and must be substituted in the place of the magnets, before any comparison can be effected. The two systems, current- and magnetic, will then be always equivalent if once equivalent.

Dr. Lodge treats this as a suggestion; but I think it is almost susceptible of demonstration. According to Weber's law, a current flowing in a closed circuit can be replaced by a simple magnetic shell of which the edge coincides with the circuit. The shell may be as thin as we please; but its *strength* must have a definite value. This law we only know to be true for air. Consider any equipotential surface of the positive magnetism on one face of the shell, at a distance from it infinitely smaller than the thickness of the shell. It passes through the substance of the shell, issuing at the edges, and covers the positive face. Similarly such an equipotential surface of the negative magnetism on the other face passes through the substance without cutting the former surface, and covers the negative face.

If on each of these surfaces we spread a surface-magnetism of which the density is the quotient of the magnetic force by 4π, then for all points outside the pair of surfaces and the shell we may replace the latter, and therefore its equivalent current-system, by the magnetic coats upon the equipotential surfaces.

Now consider any diaphragm, S, completely enclosing these surfaces and the shell. If instead of air we substitute a medium of magnetic permeability μ throughout space *outside* S, a surface-density σ is developed upon S. But the equipotential surfaces and the coats of magnetism thereon are not affected by this development; for they relate only to the en-

closed system. Hence the magnetic and current-systems, if equivalent in air, are equivalent in a medium which does not penetrate through any portion of the space occupied by the magnetic substance.

It is particularly to be noticed that, while in air any two of the infinite number of magnetic shells equivalent to two closed currents exercise the same attraction upon one another, this is no longer true when they are immersed in the medium μ. Then each pair exercises the same attraction as the two currents when the corresponding diaphragms S are drawn in the shape of the magnetic shells.

An experiment made by me in the Cavendish Laboratory last June confirms, as far as it goes, Dr. Lodge's view; but, fearing to make my letter too long for insertion, I must postpone any account of it for the present.

I am, Gentlemen,
Your obedient servant,
October 20, 1882. E. B. SARGANT.

XLIV. *Notices respecting New Books.*

The Concepts and Theories of Modern Physics. By J. B. STALLO. London: Kegan Paul and Trench. 1882.

TO write an adequate criticism of this book would involve writing a book of equal size. In the present review it is intended to make clear the standpoint from which the author speaks, and the general conclusions reached, rather than to enter into a detailed criticism of these conclusions. The failure to appreciate the position from which the author writes has already led to some misunderstanding and not a little confusion. It is stated in the preface that the work is intended "as a contribution not to physics, nor certainly to metaphysics, but to the theory of cognition." There is probably no word which is more quoted and less understood than the word "metaphysics." It is used by hardly any two writers in the same sense; and it is not too much to say that in many cases it is merely used as a term of abuse without any clear conception of its meaning. As in the present work this word is constantly used, and, moreover, as the author's main purpose is to show that certain scientific theories are in reality metaphysical, it will be important to understand precisely the signification which he puts upon the word. It is abundantly evident from the whole book that the subject which Mr. Stallo condemns, and rightly condemns, under the name of metaphysics is that which is far better designated by ontology; and in fact he himself frequently uses these terms as synonymous. The assumption of fictitious entities as causes, the belief that "the true nature of things can be discovered only by divesting them of their relations—that to be truly known they must be known as they are in themselves in their absolute essence," and in fact the whole procedure of ontology, is what is meant by

"metaphysics." Against metaphysics in this sense of the term the author, in common with many other scientific writers, wages unceasing war. Admitting the utter futility of the ontological method, it is very questionable whether this has not been overdone, whether this reiteration of abuse against ontology is not mere slaying of the slain. It is far otherwise if, after a man has clearly understood what ontology means and has been convinced of the falsity of its method, he sets himself to see whether these same errors exist in subjects non-metaphysical, and even in the reasoning of those who were loudest with their revilings. This is action, not profession, and indeed noble and useful work in the field of criticism. As Prof. Huxley most truly has said, "It is the business of criticism, not only to keep watch over the vagaries of philosophy, but to do the duty of police in the whole world of thought. Wherever it espies sophistry or superstition they are to be bidden to stand, nay, they are to be followed to their very dens and there apprehended and exterminated, as Othello smothered Desdemona, 'else she'll betray more men.'" This in truth is the task which Mr. Stallo has undertaken in the present volume, having chosen the field of science for his beat.

Besides having critical value, it is stated that the work is "intended as a contribution to the theory of cognition." Now this term, which is a translation of the German "Erkenntniss-Theorie," is but seldom used in this country in precisely the same sense which is here, in common with modern German usage, adopted. In the present work it is used in a sense very similar, if not identical, with what I have called the "New Metaphysic" (Phil. Mag. xiv. p. 75)—that is to say, in regard to method. The author, as we have seen, entirely rejects the "old metaphysic," or ontological method of inquiry—"all cognition being founded upon a recognition of relations;" and his theory of cognition seeks to discover these relations. The "thing *per se*," the "Ding an sich" or thing-in-itself of Kant, and "the absolute," as well as the assumption of other fictitious entities as the "fountain and origin of all phenomenal existence," is distinctly repudiated; and hence these conceptions form no part of his theory of cognition. From this standpoint the author proceeds to examine the validity of the reasoning upon which the mechanical theory and other scientific theories rest. It may fairly be asked, what are the qualifications of the author for this by no means easy undertaking? The title-page of the book affords no information on this point. It will therefore be probably unknown to most readers of the book in this country that the author is an American judge of no small reputation, who (I believe myself to be correct in saying) was formerly a professor of physical science.

The book itself bears witness to the author's wide acquaintance with philosophical and scientific writings. The volume may be divided into two parts. The first, consisting of eight chapters, is devoted to a rigid examination of the atomic theory, the kinetic theory of gases, and the doctrine of the conservation of energy. While admitting the value of the atomic theory as a "working hypothesis," the

author attempts to show that, as a scientific explanation of the constitution of matter, it is of little or no value. The kinetic theory of gases is condemned without reservation, as not even satisfying the conditions of a scientific theory and as based upon ontological assumptions. The doctrine of the conservation of energy is considered to be sound; and the chapter which is devoted to its examination shows an intimate acquaintance at least with the history of the subject. A detailed criticism of the validity of the author's conclusions (for there is much to be said against them) would be impossible within the limits of a review; and therefore this will not be attempted here; but one or two positive errors and misconceptions will be noticed. In the first place, it is incorrect to say (p. 23) "that with few exceptions scientific men of the present day deem the validity of the mechanical explanation of the phenomena of nature to be, not only unquestionable, but absolute, exclusive, and final. They believe that this validity is not conditioned, either by the present state of human intelligence, or by the nature and extent of the phenomena which present themselves as objects of investigation." The reverse of this is nearer to the truth. A man of science, in the capacity of a scientific investigator, is logically compelled to consider no explanation or theory as final, but to be prepared at any moment to abandon an explanation which should prove to be insufficient or at variance with facts, in favour of another which more adequately accounts for them. This is what all scientific men hold and are bound to hold, as Liebig expressed it in a passage actually quoted by the author in another part of the book, "The secret of all those who make discoveries is that they regard nothing as impossible." Throughout the book the author seems to lose sight of the necessary tentativeness of all scientific theories. Under the head of the Atomic Theory the author discusses the doctrine of the indestructibility of matter. In Chapter II. he states that the true correlate of motion is not matter, but mass; and hence this term is used where ordinarily the word matter is employed; but when discussing the indestructibility of matter in Chapter VII. we find that he uses this term, and not indestructibility of mass, which is really what chemists mean. The current doctrine is first stated, "That the constancy of mass is attested by the balance, which shows that neither fusion nor sublimation, neither generation nor corruption, can add to or detract from the weight of a body subjected to experiment. When a pound of carbon is burned, the balance demonstrates the continuing existence of this pound in the carbonic acid, which is the product of combustion, and from which the original weight of carbon may be recovered. To test the correctness of this interpretation we may be permitted slightly to vary the method of verifying it. Instead of burning the carbon, let us simply carry it to the summit of a mountain or remove it to a lower latitude: is its weight still the same? Relatively it is: it will still balance the original counterpoise. But the absolute weight is no longer the same. This appears at once if we give to the balance another form, taking a pendulum instead of a pair of scales." What the author means by talking about "absolute

weight," when he so rigidly insists "that there is nothing absolute or unconditioned in the world of reality," is by no means clear, especially as we find from the context that what he calls "absolute weight" is evidently and, indeed, necessarily based upon a relation. In any sense the expression "absolute weight" is a contradiction in terms. From this the author argues that "the ordinary statement of the fact is crude and inadequate;" and adds that it is "further necessary to remember that this weight may be infinitely reduced, without any diminution in the mass of the body weighed, by a mere change of its position in reference to the body between which and the body weighed the relation subsists." It is this very constancy of mass or quantity of matter amid all changes, of necessity relatively determined, which chemists mean to indicate by the expression "indestructibility of matter;" and the author's criticism merely amounts to a quibble about words; he has mistaken the letter for the spirit. Of the second half of the volume, Chapters IX. to XII. are devoted to the theory of Cognition, and contain an analysis of scientific ultimates, Matter, Force, Time, and Space—that is, a consideration of how we really know these, to what realities these words correspond. This being not strictly a scientific inquiry, although of much interest and importance, the results will not be considered here. In Chapters XIII. and XIV. transcendental geometry receives most severe and lengthy treatment. Lobatschewsky's non-Euclidean geometry and Riemann's doctrine of the manifoldness of space, which have occupied the attention of the most eminent mathematicians during recent years, are considered to be absurd. The same ontological error "which has given rise to the atomo-mechanical theory in physics, has led to the doctrine of pangeometry in mathematics." Even admitting the author's criticism as to the nature of space, it by no means follows that transcendental geometry is not a legitimate department of mathematics. The Nebular Hypothesis is considered in Chap. XV. as the cosmogony of the atomo-mechanical theory. The author rejects the hypothesis, first, because "all speculations respecting the universe as an unlimited whole" are fundamentally inadmissible; and, secondly, the hypothesis has proved to be at variance with a number of important astronomical facts. The last chapter of the book consists of a summary and forecast, the author concluding that "the atomo-mechanical theory cannot be the true basis of modern physics," and looking to the Conservation of Energy as a basis for the future.

On the whole the book deserves careful consideration from all physicists; for although the author has more than once mistaken the letter for the spirit, which gives some of the criticism a ludicrous aspect, and, further, has in many cases attributed to Science speculative doctrines and opinions held by individual scientific men, yet there is much acute and careful critical work in the volume. Certainly Science ought to be the first to welcome and the last to reject candid criticism of her methods and theories; for, perhaps even more than other pursuits, Science maintains *Magna est veritas et prævalebit.* WYNDHAM R. DUNSTAN.

XLV. *Intelligence and Miscellaneous Articles.*

ON DR. C. W. SIEMENS'S NEW THEORY OF THE SUN. BY M. FAYE.

IT would appear that this theory has greatly struck our physicists; for it had scarcely appeared in London when it was translated and published in France in various forms, and especially in the last number of the *Annales de Chimie et de Physique*. I suppose that the principal object of this haste was the announcement of fresh experiments which have been instituted by the author upon the chemical action of light. It is well known that, under the action of light *and with the intervention of the chlorophyll of plants*, aqueous vapour and carbonic acid are decomposed at ordinary temperatures, and brought back to the combustible form, carbon and hydrogen variously associated. Dr. Siemens has tried whether the action of the light of the Sun alone would not produce this decomposition if we submit to it, without any other intermediary, aqueous vapour and carbonic-acid gas excessively rarefied, brought for example to the vacuum of $\frac{1}{1800}$. His experiments, which, in my opinion, only require a counter-test which it would be easy to institute, have given perfectly affirmative results. Thus, the burnt gases having been brought to such a rarefaction that they no longer permitted the passage of the induction-spark, a few hours' exposure to the light of the Sun sufficed to enable the mixture to allow this spark to pass with the well-known coloration that it acquires in hydrocarburetted media *.

Regarding these beautiful experiments as decisive, Dr. Siemens has been led to inquire whether this phenomenon does not perform in the universe a part still more considerable than in vegetable life. Supposing Space to be filled with analogous gases, already burnt, the light of the Sun would revivify the combustibles hydrogen and carbon, which would then be quite ready to furnish the food of a fresh combustion.

By drawing them to himself and burning them afresh, the Sun would recuperate a good portion of the enormous heat which one is grieved to see him radiating in pure loss into celestial space.

Dr. Siemens has thus been led to put forward the following hypothesis:—Space is filled with burnt gases, aqueous vapour and carbonic acid, mixed with inert gases, nitrogen &c., pretty nearly the same as those of our atmosphere, at a pressure of $\frac{1}{2000}$. These gases are partially converted into combustibles under the action of the solar light; then, by a mechanism like that of the fan of a blower, the Sun draws them to himself, burns them, and sends them back again into space. This immense source of heat would be continually resuscitated; the only part of its radiation lost would be that which is not absorbed by the cosmical medium of a density of $\frac{1}{2000}$.

* A vacuum produced in a bell-glass into which a drop of oil of turpentine has previously been introduced, for example.

It is perfectly true that, for the physicist, air at $\frac{1}{2000}$ would be an almost absolute vacuum, so much so indeed that in such a vacuum the electric spark would no longer pass. But to the astronomer such a medium would be very dense. When we speak, in Astronomy, of the resistance of a medium or of the æther, and when by the aid of the most delicate observations and the most profound calculations, we seek for traces of this resistance, we have to do with a very different thing.

Without entering upon these discusions, I will remark that the trajectory of a cannon-ball with a velocity of 500 m. is sufficiently altered at the end of a few seconds to compel artillerists to take into account the resistance of the air in their tables.

If the air is reduced to $\frac{1}{2000}$, but the velocity of the projectile becomes that of the celestial movements, 60 times as much for example, these palpable effects will become, for a multitude of celestial projectiles of dimensions comparable to those of our cannon-balls, twice as great as in our firing-grounds, and this not merely at the end of a few years or a few centuries, but at the end of a few seconds.

In the second place, it seems to me that the celebrated English physicist has somewhat neglected to examine into the quantity of matter which he adds to the solar system. Under the influence of attraction this matter would go to unite itself with the preexisting stars, with the sun especially, and would continually augment their mass. Nothing is easier than to form an idea of this. A litre of air containing the required proportion of aqueous vapour weighs at least 1 gr. at the ordinary pressure. At a pressure of $\frac{1}{2000}$ this will be 0·0005 gr., and a cubic metre will weigh 0·0005 kilog. This being settled, if we restrict the solar system to a sphere including all the planets as far as Neptune, the weight of the excessively rarefied matter added by the hypothesis would be, in kilogrammes,

$$\tfrac{4}{3}\pi\,(6400000\times 24000\times 30)^3\times 0{\cdot}0005 \text{ kilog.}\,*$$

The actual weight of the Sun is, in kilogrammes,

$$\tfrac{4}{3}\pi\,(64000000)^3\times 5{\cdot}6\times 324000\;\dagger.$$

The first is 100,000 times as great as the second. It is therefore 100,000 times the mass of the Sun that this hypothesis adds to those of which celestial mechanics has hitherto kept so minute an account.

It is not very probable that the astronomers will adopt such hypotheses. No doubt they would be pleased to think that Nature has provided the Sun with resources to make his heat last longer; but as his final refrigeration is still, under any circumstances, a

* The first number is the radius of the earth in metres; the second the distance of our globe from the Sun in terrestrial radii; the third the distance of Neptune in parts of the distance of the Sun.

† The first number is the radius of the earth in decimetres; the second the mean density of our globe referred to that of water; the third the mass of the Sun referred to that of the earth.

tolerably distant catastrophe, they will console themselves by the thought that the things of this world, even the most beautiful, do not appear to be made to last for ever.

As to the fundamental experiments of Dr. Siemens, they will lose none of their importance in their eyes. The business is to surprise a secret of living nature, one of the laws of the organic world; and their desire will be that Dr. Siemens may pursue the course in which he has commenced so brilliantly, even though they cannot hope to have a very bright light thrown by it upon their own researches.—*Comptes Rendus*, October 9, 1882, p. 612.

ON THE CONNEXION BETWEEN THE GAS-DENSITY AND STRATUM-INTERVAL IN GEISSLER TUBES. BY DR. E. GOLDSTEIN.

Let the total length of a series of immediately consecutive strata of the positive light, of which the first is that which is next to the positive end of the tube, divided by the number of strata, be called the mean interval. The following data respecting this quantity are abstracted from experiments with dry air, hydrogen, and mixtures of the two, under such conditions of the discharge that the strata do not exhibit the to-and-fro vibrating saucer-shapes which escape any precise measurements, but appear in the so-called nebulous forms, which can be brought to a considerably greater degree of stability. In opposition to the generally current view that these clouds represent degenerations and derangements of the proper stratification, I have already* called attention to the far greater probability that they only represent the full development of that phenomenon. The thick cloudy strata stand in precisely the same relation to the thin saucer-like strata as a long-rayed tuft light at the cathode to thin films at first investing the cathode, from which, with diminishing density of the gas, the elongated rays are developed. In order to form a clear conception, I wished, further, to be able to presuppose as known that in cylindrical tubes the stratum-interval increases with increasing width of the tube†, so that, in tubes filled with air, the intervals between the individual strata, when the latter are most distinctly formed, are about equal to the diameter of the tube‡.

If, now, we determine, for cylindrical tubes of different widths inserted in the current-circuit, from an equal number of strata the mean stratum-interval J, J′, J″, . . . for any two pressures of gas d and δ, we obtain constantly

$$\frac{J_d}{J_\delta}=\frac{J'_d}{J'_\delta}=\frac{J''_d}{J''_\delta}\cdots,$$

in words:—For cylindrical tubes of different widths, the mean stratum-interval constantly varies in the same ratio between the same gas-pressures. Therefore, if, for example, in one of the tubes

* Wied. *Ann.* xii. p. 272.
† *Monatsb. der Akad. Berlin*, 1876, p. 294; Phil. Mag. [5] iv. p. 353.
‡ Wied. *Ann.* xii. p. 272.

the stratum-interval has by the rarefaction of the gas been raised to twice or three times its first-measured value, then in all the other tubes the intervals have been doubled or trebled.

The tube-diameters in my experiments varied between 2 millim. and 4 centim. The above-mentioned regularity came out, independently of whether the different cylinders formed separate vessels with two metallic electrodes each, or whether, united into a single tube, they were inserted in a line one behind another in the current.

The law is moreover found to hold good equally whether the mean interval be taken from a large or a small number of strata, provided the numbers in the different tubes be equal, although at the same time the absolute value changes. From this it can be concluded that *each single* interval also increases in accordance with the above-mentioned law.

The value of the mean interval is found with great regularity to be, in every tube, as much smaller as the number of strata from which it is derived is greater. Yet the amplitude of the undulation, multiplied by the greatest number of strata employed, never reaches the value of the smallest of the single intervals. Consequently, from the above law it follows that, if in any tube the magnitude of the mean stratum-interval for a series of gas-densities $D_1, D_2 \ldots D_n$ is known, and also, for a number of other tubes, each value of the mean interval that corresponds to any one of those densities, the number of the strata which these tubes can show at all the densities from D_1 to D_n can be calculated.

The proportion $\frac{Jd}{J\delta}=\frac{J'_d}{J'_\delta}$ permits us to conclude that the function according to which the mean interval varies with the gas-density is the same for tubes of different widths. Experiments for the purpose of ascertaining that function gave the following result:—

If the rarefactions of the gas increase in a geometrical series, the stratum-intervals are augmented also, very nearly, in a geometrical series. But the exponents of the two series are not identical; that is, the stratum-intervals are not (as was once maintained by the other side) inversely proportional to the pressure of the gas. On the contrary the measurements prove that the intervals increase much more slowly than the rarefactions—approximately at the rate of $\frac{4}{3}$ when the rarefaction is 3. I will defer more definite statements until I have determined the exponents with the greatest possible exactness, for which I am at present experimenting on a Toepler pump in the form described by von Hagen*.—*Monatsberichte der Kön. Akad. der Wissenschafteu zu Berlin*, 1881, pp. 876–878 (separate impression, communicatd by the Author).

ON THE ELASTICITY OF RAREFIED GASES. BY E. H. AMAGAT.

This subject has already been treated by Mendeleef, Kirpitchoff and Hemilian, by Silgerström, and by myself. Those researches

* Wied. *Ann.* xii. p. 425.

having led to different results, I thought it necessary to resume my experiments, considerably improving my apparatus, especially in what concerns the measurement of the pressures, which is the only difficulty peculiar to this investigation. The method employed having been already described in the *Annales de Chimie et de Physique*, t. viii. (1876), I shall only dwell on the modification which the differential barometer has undergone; it is the essentially delicate part of the apparatus, all the other parts of which have also been considerably improved. This barometer consists of a single glass tube bifurcating, at about 70 centim. above the level of the mercury in the cistern, into two wider cylindrical branches, one of which forms the barometric chamber, and the other is put into communication with the space filled with the gas the pressure of which is to be measured. The immediate result of this disposition is that there is no need to attend to the difference of temperature between the two mercurial columns, which are here joined into one at a little distance below the meniscuses. The branches of the bifurcation are prolonged upwards by stems of very small diameter, having each a glass cock, and joining again to form a single stem. The rest of the apparatus is disposed so that the manometer can be charged in place by the process generally adopted nowadays, which consists in first exhausting it with a Sprengel pump; this was worked, moreover, during all the time of the filling, so as to maintain the vacuum continually dry by the intervention of a tube containing phosphoric acid. All suction of air through the slender point was avoided by covering the surface of the mercury with a layer of sulphuric acid, which remained in the cistern during all the experiments. Above the lower single branch was a glass cock, by closing which the differential barometer could be transformed into an ordinary truncated barometer, and thus the errors due to variations of the atmospheric pressure be eliminated—which is extremely important.

In order to avoid as far as possible the errors due to refraction and capillarity, the two branches of the manometer, before being soldered to the single stem, were rounded and polished inside with the same copper mandrel, so as to be rendered perfectly cylindrical; a plane facet was then cut on the exterior, quite parallel to the generating lines of the interior cylinder. This done, the pieces were soldered, the necessary precautions being taken to keep the plane facets rigorously in the same plane.

These pieces are very difficult to obtain: a large number of them break or split, either during the rounding, or the soldering, or even after these operations are finished. The cylinders were smoothed and cut by M. Lutz; the manometers were afterwards finished by M. Alvergniat; that is to say, they were made with all the skill and perfection that could be wished for.

To eliminate capillarity-errors, an internal diameter of 2 centim. was given to the cylinders; and they were evidently perfectly equal: it was easy to verify that the mercury in them was in perfect equilibrium under the thread of the cathetometer.

I shall not dwell upon the precautions which I took with respect to the illumination of the meniscus (by means of a pencil of electric light sifted by passing through a column of water coloured with a little bichromate of potass) in order to be certain of viewing the upper part of it. Here a cause of error exists which is much more frequent than is generally thought, especially when the black silhouette of the meniscus is projected upon a bright ground.

The measurements were performed with a small cathetometer of a peculiar construction, on which $\frac{1}{200}$ millim. could be read off, and which I had made by M. Benevolo, in my private atelier, specially for these researches.

In my first investigation I descended only to 6·5 millim. pressure; this time I have often operated with pressures below 1 millim. I always arrived at the result that the deviation is of the order of magnitude of unavoidable errors. Indeed, for initial pressures of 12 millim. (in round numbers), two series composed of numerous fairly concordant results gave for the value of $\frac{pv}{p'v'}$ (v being sensibly $= 2v'$) the numbers 0·9986 and 1·0020 relative to air; for the initial pressures comprised between 3 and 4 millim. the results varied between 0·9999 and 1·0040; and for pressures near 1 millim. the extremes are 0·999 and 1·015: this divergence corresponds to an error of 15 millim. in the measurement of the pressure. All these numbers are means.

In his experiments M. Mendeleef obtained a series of products pv. This appears a more favourable condition for showing how those products vary. According to him, they go on decreasing with the pressure, starting from a certain pressure which would be 6 decim. for air. In order to prevent any delusion in this respect, it is well to observe that every sensibly constant cause of error in the estimation of the pressures, taking effect upon smaller and smaller pressures, and consequently giving a relatively greater and greater error, will produce the illusion of a regular augmentation or diminution of the products pv. This is what must result, for example, from the want of absolute vacuum in the barometric chamber, even if it be only on account of the effect produced by the mercury vapour.

In short, a minute examination of the possible errors has shown me that, even if one could attain in the readings the precision spoken of by M. Mendeleef (thousandths of a degree, and thousandths of a millimetre), that pressure would be illusory in the presence of errors proceeding from manifold causes—such as the refraction- and capillarity-errors (which, even when the precautions I have indicated are taken, are never absolutely cancelled), the error due to the compelled imperfection of the barometric chamber (which, causing all the pressures to appear a little too low, tends to produce the illusion of a negative deviation), that due to condensation of gases on the sides of the vessels or even on the mercury, &c. &c.

By admitting an error of one or two hundredths of a millimetre,

which is no exaggeration, we arrive at divergences of the order of magnitude of the deviations found; it is therefore impossible to pronounce a decision upon either the direction or even the existence of those deviations. All that we can say is that at the lowest pressures at which experiments have been made (1 millim. or even less; I have experimented at two tenths of a millimetre) no abrupt change in the law of the compressibility of gases appears to be produced; they still follow Mariotte's law, with the exception of divergences for which the experiments cannot be responsible. It is certainly possible that sufficient rarefaction, acting like a great elevation of temperature, would cause other gases to follow the law $p(v-a)=c$, as takes place for hydrogen; but there is a great distance from this to the boundary state spoken of by Mendeleef and Siljerström, in which the gases would become infinitely little compressible—a mere hypothesis, to which the numerical results of M. Siljerström do not even appear to lead, as M. Petier has already remarked in the *Journal de Physique*.

The study of carbonic acid has led me to analogous conclusions. For hydrogen the deviations found have varied between $-0{\cdot}0010$ and $0{\cdot}0028$ for initial pressures between 3 and 6 millim. in round numbers.—*Comptes Rendus de l'Académie des Sciences*, Aug. 7, 1882, t. xcv. pp. 281–284.

ON THE INFLUENCE OF TEMPERATURE UPON THE SPECTRA OF METALLOIDS. BY M. D. VAN MONCKHOVEN.

Kirchhoff and Bunsen have shown that the temperature of the flame in which a substance is reduced to vapour has no influence upon the position of the bright lines of its spectrum. When, for instance, sodium or lithium is volatilized in the flame of a spirit-lamp, or in that of the oxyhydrogen blowpipe, the lines remain the same, but their brilliancy increases with the temperature: most frequently some new thin lines appear at the elevated temperatures; but it never happens that those which have already been emitted at lower temperatures disappear. If this is always the case as regards the metallic vapours, it is never so with the lines emitted by the metalloids*. Plücker has in fact shown that oxygen, nitrogen, sulphur, selenium, &c. give two different spectra which have no line in common, according as the spectral tubes containing these substances are heated by the ordinary spark of the electrical machine or by that of a Leyden jar. He admits therefore, and with him nearly every physicist, that certain elementary bodies give, at a high temperature (Leyden jar), a spectrum different from that given by the same body at a low temperature (ordinary spark).

But numerous and varied experiments have proved that we can obtain those spectra called those of *high temperature* at very

* Hydrogen is an exception; but this gas is known to be a true metal, not only as to its chemical properties, but also as to its physical. Hydrogen bears, as regards conductivity of heat and electricity, the same relation to other gases as mercury to the other liquids.

low temperatures, and *vice versâ*. Thus, at very feeble pressures (0·001 metre), with tubes of oxygen or nitrogen and with very small Leyden jars, we obtain the spectrum which Plücker attributes to high temperatures, while the tube is scarcely warm after the experiment has last several minutes, and the brilliancy of the light emitted by the incandescent gas is very feeble. The same tube, traversed by the current of a very powerful induction-coil (without the interposition of a Leyden jar), emits, on the contrary, an extremely bright light, becomes rapidly hot, and nevertheless gives the spectrum which Plücker attributes to low temperatures.

But here is a still more decisive experiment. Let us take a tube in the form of an H with four electrodes, and filled with nitrogen*, oxygen, or one of those gases (or vapours) which give two spectra. Through this tube let us pass at the same time the currents of two induction-coils, of which one has a Leyden jar interposed. *We shall observe the two spectra superposed—the spectrum assigned to elevated temperatures (Leyden jar), and the spectrum assigned to low temperatures (ordinary spark).*

According to Plücker's hypothesis, the gas would have, at the same physical instant, two different temperatures, which is inadmissible.

It may perhaps be objected that, the interrupters of the two coils not working strictly in unison, the perception of the two spectra is due to the persistence of the images upon the retina. But this is not the case, as some tubes, especially with oxygen, give forth light for several tenths of a second after the current has been interrupted.

We attribute the change in the spectra given by these metalloids to a vibratory state peculiar to their molecules, directly depending upon the nature of the electricity employed. Thus, a tube of highly rarefied hydrogen gas, submitted to the action of ordinary sparks, presents quite a different aspect from the same tube submitted to the action of the condensed spark.

Highly rarefied gases, traversed by the continuous current of the battery, or by a current interrupted by sparks (induction-coil), present a dynamical state well known under the name of *stratification*. But this stratification differs entirely, according as we employ the ordinary spark, the condensed spark, or the continuous current of a battery of very high tension.

We shall see in further communications that with each different behaviour of an incandescent gas (alteration in the stratification, colour of the light emitted, &c.) there is always a corresponding modification, and often an entire change, in its spectral lines—an effect assuredly independent of the temperature.—*Comptes Rendus*, Sept. 18, 1882.

ON A THERMOSCOPIC METHOD FOR THE DETERMINATION OF THE OHM. BY G. LIPPMANN.

It will be remembered that Mr. Joule† employed a calorimetric

* Nitrogen in the electric arc gives a different spectrum from that given by Geissler's tubes or the spark in air.

† Reports of the Committee &c., pp. 175–190 (London, 1873).

method for the determination of the ohm. The method which I am about to describe differs from that of the eminent physicist in not requiring the quantities of heat to be measured or the mechanical equivalent of heat E to be known. This last point is not unimportant; for in Joule's calorimetric method the final approximation is limited by the uncertainty at present existing respecting the exact value of the number E; that is to say, the possible error is near $\frac{1}{100}$.

The wire of which I wish to know the resistance is placed in the middle of a vessel arranged as a calorimeter in the centre of an enclosure with a constant temperature. An electric current is passed into the wire, and its intensity i measured. I wait until, in consequence of the heat liberated by the current, the vessel attains a stationary temperature; I leisurely ascertain that it is so by employing a thermometer, or, rather, a sensitive *thermoscope*, placed inside the vessel. This done, I interrupt the current, and then set in action a motor which produces friction in the midst of the vessel that already contains the wire. The heat evolved by the friction is substituted for that just before evolved by the current. I manage so that the stationary temperature resumes its former value; I then have $ri^2=T$, T being the work expended, whence the value of r. It is scarcely necessary to add that the friction-apparatus must remain in the vessel which contains it, even when it is not in operation, and be furnished with the known arrangements for measuring T. It is also more convenient to commence by the friction-experiment, and afterwards to regulate the intensity i so as to recover the same stationary temperature. Lastly, it may be advantageous, for apparatus of large capacity, to replace the observation of the stationary temperature by that of the velocity of the heating.

In the form given to it by Joule in 1867, the calorimetric method of the English physicist rests equally upon the measurement of i and the measurement of a mechanical work, namely the work done at the time of the determination of E. Moreover, it involves two calorimetric measurements, which are to be mutually eliminated from the final result—namely, the calorimetric measurement which accompanies the determination of E, and that which accompanies the passage of the electric current. These intermediate determinations bring in their causes of error and their corrections, owing to the imperfections of the calorimeters employed in making them. I dispense with them by taking care to expend the work T and the electric energy ri^2 in one and the same caloriscopic vessel. It becomes as needless to ascertain the quantity of heat evolved in that vessel as to ascertain the weight of the tare in a double weighing; and the advantage obtained appears analogous to that which there would be in replacing two successive single weighings, made with different balances and different weights, by a Borda's double weighing.—*Comptes Rendus de l'Académie des Sciences,* Oct. 9, 1882, t. xcv. pp. 634, 635.

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XLVI. *On Variations in the Vertical due to Elasticity of the Earth's Surface.* By G. H. Darwin, *F.R.S., formerly Fellow of Trinity College, Cambridge**.

1. *On the Mechanical Effects of Barometric Pressure on the Earth's Surface.*

THE remarks of Signore de Rossi, on the observed connexion between barometric storms and the disturbance of the vertical, have led me to make the following investigation of the mechanical effects which are caused by variations of pressure acting on an elastic surface. The results seem to show that the direct measurement of the lunar disturbance of gravity must for ever remain impossible.

The practical question is to estimate the amount of distortion to which the upper strata of the earth's mass are subjected, when a wave of barometric depression or elevation passes over the surface. The solution of the following problem should give us such an estimate.

Let an elastic solid be infinite in one direction, and be bounded in the other direction by an infinite plane. Let the surface of the plane be everywhere acted on by normal pressures and tractions, which are expressible as a simple harmonic function of distances measured in some fixed direction along

* Appendix to the Second Report of the Committee of the British Association on the Lunar Disturbance of Gravity. Read at the Meeting at Southampton, August 1882. Communicated by the Author.

the plane. It is required to find the form assumed by the surface, and generally the condition of internal strain.

This is clearly equivalent to the problem of finding the distortion of the earth's surface produced by parallel undulations of barometric elevation and depression. It is but a slight objection to the correctness of a rough estimate of the kind required, that barometric disturbances do not actually occur in parallel bands, but rather in circles. And when we consider the magnitude of actual terrestrial storms, it is obvious that the curvature of the earth's surface may be safely neglected.

This problem is mathematically identical with that of finding the state of stress produced in the earth by the weight of a series of parallel mountains. The solution of this problem has recently been published in a paper by me in the 'Philosophical Transactions' (part ii. 1882, pp. 187–230); and the solution there found may be adapted to the present case in a few lines.

The problem only involves two dimensions. If the origin be taken in the mean horizontal surface, which equally divides the mountains and valleys, and if the axis of z be horizontal and perpendicular to the mountain-chains, and if the axis of x be drawn vertically downwards, then the equation to the mountains and valleys is supposed to be

$$x = -h \cos \frac{z}{b},$$

so that the wave-length from crest to crest of the mountain-ranges is $2\pi b$.

The solution may easily be found from the analysis of section 7 of the paper referred to. It is as follows:—

Let α, γ be the displacements at the point x, z vertically downwards and horizontally (α has here the opposite sign to the α of (44)). Let w be the density of the rocks of which the mountains are composed, g gravity, υ modulus of rigidity; then

$$\left.\begin{aligned} \alpha &= \frac{1}{2\upsilon} b \left[x \frac{dW}{dx} - W \right], \\ \gamma &= \frac{1}{2\upsilon} bx \frac{dW}{dz}, \\ \text{where} \quad W &= -gwh\, e^{-x/b} \cos \frac{z}{b}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

From these we have at once

$$\left.\begin{aligned}\alpha &= \frac{gwh}{2v}\, b\left(1+\frac{x}{b}\right)e^{-x/b}\cos\frac{z}{b},\\ \gamma &= \frac{gwh}{2v}\, xe^{-x/b}\sin\frac{z}{b},\\ \frac{d\alpha}{dz} &= -\frac{gwh}{2v}\left(1+\frac{x}{b}\right)e^{-x/b}\sin\frac{z}{b}.\end{aligned}\right\} \quad . \quad . \quad . \quad (2)^{*}$$

The first of these gives the vertical displacement, the second the horizontal, and the third the inclination to the horizon of strata primitively plane.

At the surface,

$$\left.\begin{aligned}\alpha &= \frac{gwh}{2v}\, b\cos\frac{z}{b}, \quad \gamma=0,\\ \frac{d\alpha}{dz} &= -\frac{gwh}{2v}\sin\frac{z}{b}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Hence the maximum vertical displacement of the surface is $\pm gwhb/2v$, and the maximum inclination of the surface to the horizon is

$$\pm \operatorname{cosec} 1'' \times gwh/2v \text{ seconds of arc.}$$

Before proceeding further, I shall prove a very remarkable relation between the slope of the surface of an elastic horizontal plane and the deflection of the plumb-line caused by the direct attraction of the weight producing that slope. This relation was pointed out to me by Sir William Thomson, when I told him of the investigation on which I was engaged; but I am alone responsible for the proof as here given. He writes that he finds that it is not confined simply to the case where the solid is incompressible; but in this paper it will only be proved for that case.

Let there be positive and negative matter distributed over the horizontal plane according to the law $wh\cos(z/b)$: this

* It is easy to verify that these values of α and γ, together with the value $p = gwh\,e^{-x/b}\cos z/b$ for the hydrostatic pressure, satisfy all the conditions of the problem, by giving normal pressure $gwh\cos z/b$ at the free surface of the infinite plane, and satisfying the equations of internal equilibrium throughout the solid. I take this opportunity of remarking that the paper from which this investigation is taken contains an error, inasmuch as the hydrostatic pressure is erroneously determined in section 1. The term $-W$ should be added to the pressure as determined in (3). This adds W to the normal stresses P, Q, R throughout the paper, but leaves the difference of stresses (which was the thing to be determined) unaffected. If the reader should compare the stresses as determined from the values of α, γ in the text above, and from the value of p given in this note, with (38) of the paper referred to, he is warned to remember the missing term W.

forms, in fact, harmonic mountains and valleys on the infinite plane. We require to find the potential and attraction of such a distribution of matter.

Now the potential of an infinite straight line, of line-density ρ, at a point distant d from it, is well known to be $-2\mu\rho \log d$, where μ is the attraction between unit masses at unit distance apart. Hence the potential V of the supposed distribution of matter at the point x, z is given by

$$V = -2\mu wh \int_{-\infty}^{+\infty} \cos\frac{\zeta}{b} \log \sqrt{\{x^2 + (\zeta - z)^2\}} d\zeta$$

$$= -\mu whb \left\{ \left[\sin\frac{\zeta}{b} \log \{x^2 + (\zeta - z)^2\} \right]_{-\infty}^{+\infty} - 2\int_{-\infty}^{+\infty} \frac{(\zeta - z)\sin(\zeta/b)}{x^2 + (\zeta - z)^2} d\zeta \right\}$$

It is not hard to show that the first term vanishes when taken between the limits.

Now put $t = \dfrac{\zeta - x}{x}$ so that $\sin\dfrac{\zeta}{b} = \sin\dfrac{tx}{b}\cos\dfrac{z}{b} + \cos\dfrac{tx}{b}\sin\dfrac{z}{b}$, and we have

$$V = 2\mu whb \int_{-\infty}^{+\infty} \left(\sin\frac{tx}{b}\cos\frac{z}{b} + \cos\frac{tx}{b}\sin\frac{z}{b} \right) \frac{tdt}{1+t^2}.$$

But it is known* that

$$\int_{-\infty}^{+\infty} \frac{t \sin ct\, dt}{1+t^2} = \pi e^{-c}, \quad \int_{-\infty}^{+\infty} \frac{t \cos ct}{1+t^2} dt = 0.$$

Therefore

$$V = 2\pi\mu whb e^{-x/b} \cos\frac{z}{b}.$$

If g be gravity, a earth's radius, and δ earth's mean density,

$$2\pi\mu = \frac{3g}{2a\delta}.$$

And

$$V = \frac{3gwh}{2a\delta} b e^{-x/b} \cos\frac{z}{b}. \quad \ldots\ldots \quad (4)$$

The deflection of the plumb-line at any point on the surface denoted by $x = 0$, and z, is clearly dV/gdz, when $x = 0$. Therefore

$$\text{the deflection} = -\frac{1}{g} \times \frac{3gwh}{2a\delta} \sin\frac{z}{b}. \quad \ldots \quad (5)$$

But from (2) the slope (or $\dfrac{d\alpha}{dz}$, when z is zero) is

$$-\frac{gwh}{2v} \sin\frac{z}{h}.$$

* See Todhunter's 'Integ. Calc.'; chapter on "Definite Integrals."

Therefore deflection bears to slope the same ratio as v/g to $\frac{1}{3}a\delta$. This ratio is independent of the wave-length $2\pi b$ of the undulating surface, of the position of the origin, and of the azimuth in the plane of the line normal to the ridges and valleys. Therefore the proposition is true of any combination whatever of harmonic undulations; and as any inequality may be built up of harmonic undulations, it is generally true of inequalities of any shape whatever.

Now $a = 6{\cdot}37 \times 10^8$ centim., $\delta = 5\frac{2}{3}$; and $\frac{1}{3}a\delta = 12{\cdot}03 \times 10^8$ grammes per square centimetre. The rigidity of glass in gravitation-units ranges from $1{\cdot}5 \times 10^8$ to $2{\cdot}4 \times 10^8$. Therefore the slope of a very thick slab of the rigidity of glass, due to a weight placed on its surface, ranges from 8 to 5 times as much as the deflection of the plumb-line due to the attraction of that weight. Even with rigidity as great as steel (viz. about 8×10^8), the slope is $1\frac{1}{2}$ times as great as the deflection.

A practical conclusion from this is that, in observations with an artificial horizon, the disturbance due to the weight of the observer's body is very far greater than that due to the attraction of his mass. This is in perfect accordance with the observations made by my brother and me with our pendulum in 1881, when we concluded that the warping of the soil by our weight when standing in the observing-room was a very serious disturbance, whilst we were unable to assert positively that the attraction of weights placed near the pendulum was perceptible. It also gives emphasis to the criticism we have made on M. Plantamour's observations—namely, that he does not appear to take special precautions against the disturbance due to the weight of the observer's body.

We must now consider the probable numerical values of the quantities involved in the barometric problem, and the mode of transition from the problem of the mountains to that of barometric inequalities.

The modulus of rigidity in gravitation-units (say grammes weight per square centimetre) is v/g. In the problem of the mountains, wh is the mass of a column of rock of one square centimetre in section and of length equal to the height of the crests of the mountains above the mean horizontal plane. In the barometric problem, wh must be taken as the mass of a column of mercury of a square centimetre in section and equal in height to a half of the maximum range of the barometer.

This maximum range is, I believe, nearly two inches, or, let us say, 5 centim.

The specific gravity of mercury is 13·6; and therefore $wh = 34$ grammes.

The rigidity of glass is from 150 to 240 million grammes

per square centimetre, that of copper 540, and of steel 843 millions.

I will take $v/g=3\times10^8$; so that the superficial layers of the earth are assumed to be more rigid than the most rigid glass. It will be easy to adjust the results afterwards to any other assumed rigidity.

With these data we have

$$\frac{gwh}{2v}=\frac{5\cdot67}{10^8};$$

also

$$\frac{648{,}000}{\pi}\times\frac{5\cdot67}{10^8}=0''\cdot0117.$$

It seems not unreasonable to suppose that 1500 miles ($2\cdot4\times10^8$ centim.) is the distance from the place where the barometer is high (the centre of the anti-cyclone) to that where it is low (the centre of the cyclone). Accordingly the wave-length of the barometric undulation is $4\cdot8\times10^8$ centim., and $b=4\cdot8\times10^8\div6\cdot28$ centim., or, say, $b=\cdot8\times10^8$ centim.

Thus, with these data,

$$\frac{gwh}{2v}b=4\cdot5 \text{ centim.}$$

We thus see that the ground is 9 centim. higher under the barometric depression than under the elevation.

If the sea had time to attain its equilibrium slope, it would stand $5\times13\cdot6$, or 68 centim. lower under the high pressure than under the low. But as the land is itself depressed 9 centim., the sea would apparently only be depressed 59 centim. under the high barometer.

It is probable that, in reality, the larger barometric inequalities do not linger quite long enough over particular areas to permit the sea to attain everywhere its due slope, and therefore the full difference of water-level can only be attained occasionally.

On the other hand, the elastic compression of the ground must take place without any sensible delay. Thus it seems probable that the elastic compression of the ground must exercise a very sensible effect in modifying the apparent depression or elevation of the sea under high and low barometer.

It does not appear absolutely chimerical that at some future time, when both tidal and barometric observations have attained to great accuracy, an estimate might thus be made of the average modulus of rigidity of the upper 500 miles of the earth's mass.

Even in the present condition of barometric and tidal infor-

mation, it might be interesting to make a comparison between the computed height of tide and the observed height, in connexion with the distribution of barometric pressure. It is probable that India would be the best field for such an attempt, because the knowledge of Indian tides is more complete than that for any other part of the world. On the other hand, we shall see in the following section that tidal observations on coast-lines of continents are liable to disturbance, so that an oceanic island would be a more favourable site.

It has already been shown that the maximum apparent deflection of the plumb-line, consequent on the elastic compression of the earth, amounts to $0''\cdot0117$; and this is augmented to $0''\cdot0146$, when we include the true deflection due to the attraction of the air. It is worthy of remark that this result is independent of the wave-length of the barometric inequality, and thus we get rid of one of the conjectural data.

Thus, if we consider the two cases of high pressure to right and low to left, and of low pressure to right and high to left, we see that there will be a difference in the position of the plumb-line relatively to the earth's surface of $0''\cdot0292$. Even if the rigidity of the upper strata of the earth were as great as that of steel, there would still be a change of $0''\cdot011$.

A deflection of magnitude such as $0''\cdot03$ or $0''\cdot01$ would have been easily observable with our instrument of last year; for we concluded that a change of $\frac{1}{200}$ of a second could be detected when the change occurred rapidly.

It was stated in our previous Report that at Cambridge the calculated amplitude of oscillation of the plumb-line due directly to lunar disturbance of gravity amounts to $0''\cdot0216$. Now, as this is less than the amplitude due jointly to elastic compression and attraction, with the assumed rigidity (300 millions) of the earth's strata, and only twice the result if the rigidity be as great as that of steel, it follows almost certainly that from this cause alone the measurement of the lunar disturbance of gravity mnst be impossible with any instrument on the earth's surface.

Moreover the removal of the instrument to the bottom of the deepest known mine would scarcely sensibly affect the result, because the flexure of the strata at a depth so small, compared with the wave-length of barometric inequalities, is scarcely different from the flexure of the surface.

The diurnal and periodic oscillations of the vertical observed by us were many times as great as those which have just been computed; and therefore it must not be supposed that more than a fraction, say perhaps a tenth, of those oscillations was due to elastic compression of the earth.

The Italian observers could scarcely with their instruments detect deflections amounting to $\frac{1}{100}$ of a second; so that the observed connexion between barometric oscillation and seismic disturbance must be of a different kind.

It is not surprising that in a volcanic region the equalization of pressure, between imprisoned fluids and the external atmosphere, should lead to earthquakes.

If there is any place on the earth's surface free from seismic forces, it might be possible (if the effect of tides as computed in the following section could be eliminated) with some such instrument as ours, placed in a deep mine, to detect the existence of barometric disturbance many hundreds of miles away. It would of course for this purpose be necessary to note the positions of the sun and moon at the times of observation, and to allow for their attraction.

2. *On the Disturbance of the Vertical near the Coasts of Continents due to the Rise and Fall of the Tide.*

Consider the following problem:—

On an infinite horizontal plane, which bounds in one direction an infinite incompressible elastic solid, let there be drawn a series of parallel straight lines, distance l apart. Let one of these be the axis of y, let the axis of z be drawn in the plane, perpendicular to the parallel lines, and let the axis of x be drawn vertically downwards through the solid.

At every point of the surface of the solid, from $z=0$ to l, let a normal pressure $gwh(1-2z/l)$ be applied; and from $z=0$ to $-l$ let the surface be free from forces. Let the same distribution of force be repeated over all the pairs of strips into which the surface is divided by the system of parallel straight lines. It is required to determine the strains caused by these forces.

Taking the average over the whole surface, there is neither pressure nor traction, since the total traction on the half-strips subject to traction is equal to the total pressure on the half-strips subject to pressure.

The following is the analogy of this system with that which we wish to discuss: the strips subject to no pressure are the continents, the alternate ones are the oceans, g is gravity, w the density of water, and h the height of tide above mean water on the coast-line.

We require to find the slope of the surface at every point, and the vertical displacement.

It is now necessary to bring this problem within the range of the results used in the last section. In the first place, it is convenient to consider the pressures and tractions as caused by

mountains and valleys whose outline is given by $x=-h(1-2z/l)$ from $z=0$ to l, and $x=0$ from $z=0$ to $-l$. To utilize the analysis of the last section, it is necessary that the mountains and valleys should present a simple-harmonic outline. Hence the discontinuous function must be expanded by Fourier's method. Known results of that method render it unnecessary to have recourse to the theorem itself. It is known that

$$\pm\tfrac{1}{2}\pi-\tfrac{1}{2}\theta=\sin\theta+\tfrac{1}{2}\sin 2\theta+\tfrac{1}{3}\sin 3\theta+\ldots$$

$$-\tfrac{1}{2}\theta=-\sin\theta+\tfrac{1}{2}\sin 2\theta-\tfrac{1}{3}\sin 3\theta+\ldots$$

$$\tfrac{1}{2}\pi\mp\theta=\frac{4}{\pi}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\},$$

the upper sign being taken for values of θ between the infinitely small positive and $+\pi$, and the lower for values between the infinitely small negative and $-\pi$.

Adding these three series together, we have

$$2\{\tfrac{1}{2}\sin 2\theta+\tfrac{1}{4}\sin 4\theta+\ldots\}+\frac{4}{\pi}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\}$$

equal to $\pi-2\theta$ from $\theta=0$ to $+\pi$, and equal to zero from $\theta=0$ to $-\pi$. Hence the required expansion of the discontinuous function is

$$\left.\begin{aligned}&-\frac{2h}{\pi}\{\tfrac{1}{2}\sin 2\theta+\tfrac{1}{4}\sin 4\theta+\ldots\},\\&-\frac{4h}{\pi^2}\left\{\cos\theta+\frac{1}{3^2}\cos 3\theta+\frac{1}{5^2}\cos 5\theta+\ldots\right\},\end{aligned}\right\}\quad . \quad (6)$$

where

$$\theta=\frac{\pi z}{l}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

for it vanishes from $z=-l$ to 0, and is equal to $-h(1-2z/l)$ from $z=0$ to $+l$.

Now, looking back to the analysis of the preceding section, we see that, if the equation to the mountains and valleys had been $x=-h\sin(z/b)$, α would have had the same form as in (2), but of course with sine for cosine, and γ would have changed its sign and a cosine would have stood for the sine. Applying then the solution (2) to each term of our expansion separately, and only writing down the solution for the surface at which $x=0$, we have at once that $\gamma=0$, and

$$\left.\begin{aligned}\alpha=&\frac{gwh}{\pi v}\frac{l}{\pi}\left\{\frac{1}{2^2}\sin 2\theta+\frac{1}{4^2}\sin 4\theta+\frac{1}{6^2}\sin 6\theta+\ldots\right\},\\&+\frac{gwh}{\pi v}\cdot\frac{2l}{\pi^2}\left\{\cos\theta+\frac{1}{3^3}\cos 3\theta+\frac{1}{5^3}\cos 5\theta+\ldots\right\}.\end{aligned}\right\}. \quad (8)$$

The slope of the surface is $\frac{d\alpha}{dz}$ or $\frac{\pi}{l}\frac{d\alpha}{d\theta}$; thus

$$\left.\begin{aligned}\frac{d\alpha}{dz}&=\frac{gwh}{\pi v}\{\tfrac{1}{2}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\tfrac{1}{6}\cos 6\theta+\dots\}\\&\quad-\frac{gwh}{\pi v}\cdot\frac{2}{\pi}\left\{\sin\theta+\frac{1}{3^2}\sin 3\theta+\frac{1}{5^2}\sin 5\theta+\dots\right\}.\end{aligned}\right\}\quad .\ (9)$$

The formulæ (8) and (9) are the required expressions for the vertical depression of the surface and for the slope.

It is interesting to determine the form of surface denoted by these equations. Let us suppose, then, that the units are so chosen that $gwhl/\pi^2 v$ may be equal to one. Then (8) and (9) become

$$\alpha=\frac{1}{2^2}\sin 2\theta+\frac{1}{4^2}\sin 4\theta+\dots+\frac{2}{\pi}\left\{\frac{1}{1^3}\cos\theta+\frac{1}{3^3}\cos 3\theta+\dots\right\}\ (10)$$

$$\frac{d\alpha}{d\theta}=\tfrac{1}{2}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\dots-\frac{2}{\pi}\left\{\frac{1}{1^2}\sin\theta+\frac{1}{3^2}\sin 3\theta+\dots\right\}.\ \ 11)$$

When θ is zero or $\pm\pi$, $d\alpha/d\theta$ becomes infinite, which denotes that the tangent to the warped horizontal surface is vertical at these points. The verticality of these tangents will have no place in reality, because actual shores shelve, and there is not a vertical wall of water when the tide rises, as is supposed to be the case in the ideal problem. We shall, however, see that in practical numerical application, the strip of sea-shore along which the solution shows a slope of more than $1''$ is only a small fraction of a millimetre. Thus this departure from reality is of no importance whatever.

When $\theta=0$ or $\pm\pi$,

$$\alpha=\frac{2}{\pi}\left\{\frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\dots\right\}=\frac{2}{\pi}\times 1{\cdot}052={\cdot}670,\ (12)$$

being + when $\theta=0$, and − when $\theta=\pm\pi$.

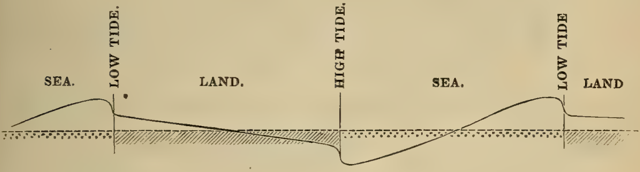
When $\theta=\pm\frac{1}{2}\pi$, α vanishes; and therefore midway in the ocean and on the land there are nodal lines, which always remain in the undisturbed surface, when the tide rises and falls. At these nodal lines, defined by $\theta=\pm\frac{1}{2}\pi$,

$$\frac{d\alpha}{d\theta}=-\tfrac{1}{2}\log_e 2\mp\frac{2}{\pi}\left\{\frac{1}{1^3}-\frac{1}{3^3}+\frac{1}{5^3}-\dots\right\}$$

$$=-{\cdot}3466\mp{\cdot}6168=-{\cdot}9634\text{ and }+{\cdot}2702.$$

Thus the slope is greater at mid-ocean than at mid-land. By assuming θ successively as $\frac{1}{6}\pi$, $\frac{1}{4}\pi$, $\frac{1}{3}\pi$, and summing arithmetically the strange series which arise, we can, on pay-

ing attention to the manner in which the signs of the series occur, obtain the values of α corresponding to 0, $\pm\frac{1}{6}\pi$, $\pm\frac{1}{4}\pi$, $\pm\frac{2}{6}\pi$, $\pm\frac{3}{6}\pi$, $\pm\frac{4}{6}\pi$, $\pm\frac{5}{6}\pi$, $\pm\frac{6}{6}\pi$. The resulting values, together with the slopes as obtained above, are amply suffi- ient for drawing a figure, as shown annexed.



The straight line is a section of the undisturbed level, the shaded part being land, and the dotted sea. The curve shows the distortion, when warped by high and low tide as indicated.

The scale of the figure is a quarter of an inch to $\frac{1}{6}\pi$ for the abscissas, and a quarter of an inch to unity for the ordinates; it is of course an enormous exaggeration of the flexure actually possibly due to tides.

It is interesting to note that the land-regions remain very nearly flat, rotating about the nodal line, but with slight curvature near the coasts. It is this curvature, scarcely perceptible in the figure, which is of most interest for practical application.

The series (8) and (9) are not convenient for practical calculation in the neighbourhood of the coast, and they must be reduced to other forms. It is easy, by writing the cosines in their exponential form, to show that

$$\cos\theta+\tfrac{1}{2}\cos 2\theta+\tfrac{1}{3}\cos 3\theta+\ldots=-\log_e(\pm 2\sin\tfrac{1}{2}\theta), \quad . \quad (13)$$

$$\cos\theta-\tfrac{1}{2}\cos 2\theta+\tfrac{1}{3}\cos 3\theta-\ldots=\log_e(2\cos\tfrac{1}{2}\theta), \quad . \quad . \quad (14)$$

where the upper sign in (13) is to be taken for positive values of θ and the lower for negative.

For the small values of θ with which alone we are at present concerned, the series (13) becomes $-\log_e(\pm\theta)$ and the lower $\log_e 2$.

Taking half the difference and half the sum of the two series, we have

$$\tfrac{1}{2}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\ldots\ldots=-\tfrac{1}{2}\log(\pm\theta)-\tfrac{1}{2}\log 2, \quad . \quad (15)$$

$$\cos\theta+\tfrac{1}{3}\cos 3\theta+\tfrac{1}{5}\cos 5\theta+=-\tfrac{1}{2}\log(\pm\theta)+\tfrac{1}{2}\log 2. \quad . \quad (16)$$

Integrating (16) with regard to θ, and observing that the

constant introduced on integration is zero, we have

$$\sin\theta+\frac{1}{3^2}\sin 3\theta+\frac{1}{5^2}\sin 5\theta+\ldots=-\tfrac{1}{2}\theta[\log(\pm\theta)-1]+\tfrac{1}{2}\theta\log 2. \quad . \quad . \quad (17)$$

Then, from (15) and (17),

$$\left.\begin{aligned}&\tfrac{1}{4}\cos 2\theta+\tfrac{1}{4}\cos 4\theta+\ldots-\frac{2}{\pi}\left\{\sin\theta+\frac{1}{3^2}\sin 3\theta+\ldots\right\}\\ &=-\tfrac{1}{2}\left(1-\frac{2\theta}{\pi}\right)\log(\pm\theta)-\tfrac{1}{2}\left(1+\frac{2\theta}{\pi}\right)\log 2-\frac{\theta}{\pi}.\end{aligned}\right\} \quad (18)$$

Integrating (15), and observing that the constant is zero, we have

$$\frac{1}{2^2}\sin 2\theta+\frac{1}{4^2}\sin 4\theta+\ldots=-\tfrac{1}{2}\theta[\log(\pm\theta)-1]-\tfrac{1}{2}\theta\log 2. \quad (19)$$

Integrating (17), and putting in the proper constant to make the left side vanish when $\theta=0$, we have

$$\begin{aligned}&\frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\ldots-\left(\frac{1}{1^3}\cos\theta+\frac{1}{3^3}\cos 3\theta+\ldots\right)\\ &\qquad=-\tfrac{1}{4}\theta^2\log(\pm\theta)+\tfrac{1}{4}\theta^2\left(\tfrac{3}{2}+\log 2\right). \quad . \quad . \quad (20)\end{aligned}$$

For purposes of practical calculation, θ may be taken as so small that the right-hand side of (18) reduces to $-\frac{1}{2}\log(\pm 2\theta)$, and the right-hand sides of (19) and (20) to zero.

Hence, by (8) and (9), we have in the neighbourhood of the coast,

$$\left.\begin{aligned}\alpha&=\frac{gwh}{\pi v}\times\frac{2l}{\pi^2}\left[\frac{1}{1^3}+\frac{1}{3^3}+\frac{1}{5^3}+\ldots\right],\\ &=\frac{gwh}{\pi v}\times\frac{l}{\pi^2}\times 2{\cdot}1037,\\ \frac{d\alpha}{dz}&=-\frac{gwh}{2\pi v}\log_e 10\log_{10}\frac{2\pi z}{l}.\end{aligned}\right\} \quad . \quad . \quad (21)$$

I shall now proceed to compute from the formulæ (21) the depression of the surface and the slope, corresponding to such numerical data as seem most appropriate to the terrestrial oceans and continents.

Considering that the tides are undoubtedly augmented by kinetic action, we shall be within the mark in taking h as the semi-range of equilibrium tide. At the equator the lunar tide has a range of about 53 centim., and the solar tide is very nearly half as much. Therefore at spring-tides we may take $h=40$ centim. It must be noticed that the highness of the tides (say 15 or 20 feet) near the coast is due to the shallowing of the water, and it would not be just to take such values

as representing the tides over large areas; w, the density of the water, is of course unity.

If we suppose it is the Atlantic Ocean and the shores of Europe with Africa, and of North and South America, which are under consideration, it is not unreasonable to take l as 3900 miles, or $6{\cdot}28 \times 10^8$ centim. Then $2\pi z/l = z \times 10^{-8}$.

Taking v/g as 3×10^8 (that is to say, assuming a rigidity greater than that of glass), we have for the slope in seconds of arc, at a distance z from the sea-shore,

$$\operatorname{cosec} 1'' \times \frac{40}{2\pi \times 3 \times 10^8} \times \log_e 10 \times (8 - \log_{10} z)$$
$$= 0''{\cdot}01008\,(8 - \log_{10} z). \quad . \quad . \quad . \quad (22)$$

From this the following table may be computed by simple multiplication:—

Distance from mean water-mark.		Slope.
1 centim.	= 1 centim.	$0''{\cdot}0806$
10 ,,	= 10 ,,	·0706
10^2 ,,	= 1 metre	·0605
10^3 ,,	= 10 metres	·0504
10^4 ,,	= 100 ,,	·0403
10^5 ,,	= 1 kilom.	·0302
10^6 ,,	= 10 ,,	·0202
2×10^6 centim.	= 20 ,,	·0170
5×10^6 ,,	= 50 ,,	·0131
10^7 centim.	= 100 ,,	·0101

On considering the formula (22), it appears that z must be a very small fraction of a millimetre before the slope becomes even as great as $1'$. This proves that the rounded nick in the surface, which arises from the discontinuity of pressure at our ideal mean water-mark, is excessively small; and the vertical displacement of the surface is sensibly the same, when measured in centimetres, on each side of the nick, in accordance with the first of (21).

The result (5) of section 1 shows that, with rigidity 3×10^8, the true deflection of plumb-line due to attraction of the water is a quarter of the slope. Hence an observer in a gravitational observatory at distance z from mean water-mark, would note deflections from the mean position of the vertical $1\frac{1}{4}$ times as great as those computed above; and as high water changes to low, there would be oscillations of the vertical $2\frac{1}{2}$ times as great. We thus get the practical results in the following table:—

Distance of observatory from mean water-mark.	Amplitude of apparent oscillation of the vertical.
10 metres	0″·126
100 „	·101
1 kilom.	·076
10 „	·050
20 „	·042
50 „	·035
100 „	·025

It follows, from the calculations made for tracing the curve, that halfway across the continent (that is to say, 3142 kilometres from either coast) the slope is

$$\frac{648{,}000}{\pi} \times \frac{gwh}{\pi v} \times \cdot 2703 \text{ second of arc } = 0'' \cdot 00237,$$

and the range of apparent oscillation is 0″·006.

In these calculations the width of the sea is taken as 6283 kilometres. If the sea be narrower, then, to obtain the same deflections of the plumb-line, the observatory must be moved nearer the sea in the same proportion as the sea is narrowed. If, for example, the sea were 3142 kilometres wide, then at 10 kilometres from the coast the apparent amplitude of deflection would be 0″·042. If the range of tide is greater than that here assumed (viz. 80 centim.), the results must be augmented in the same proportion. And, lastly, if the rigidity of the rock be greater or less than the assumed value (viz. 3×10^8), the part of the apparent deflection depending on slope must be diminished or increased in the inverse proportion to the change in rigidity.

I think there can be little doubt that in narrow seas the tides are generally much greater than those here assumed; and it is probable that at a gravitational observatory actually on the sea-shore on the south coast of England, apart from seismic changes, perceptible oscillations of the vertical would be noted.

Sir William Thomson has made an entirely independent estimate of the probable deflection of the plumb-line at a sea-side gravitational observatory*. He estimates the attraction of a slab of water 10 feet thick (the range of tide), 50 miles broad perpendicular to the coast, and 100 miles long parallel with coast, on a plummet 100 yards from the low-water mark, and opposite the middle of the 100 miles of length. He thinks this estimate would very roughly represent the state of things

* Thomson and Tait's 'Natural Philosophy,' § 818.

say at St. Alban's Head. He finds, then, that the deflection of the plumb-line as high tide changes to low would be $\frac{1}{4000000}$ of the unit angle, or 0″·050. The general theorem proved above, as to the proportionality of slope to attraction, shows that, with rigidity 3×10^8 for the rocks of which the earth is formed, the apparent deflection of the plumb-line would amount to 0″·25.

It is just possible that a way may in this manner be opened for determining the modulus of rigidity of the upper 100 or 200 miles of the earth's surface, although the process would be excessively laborious. The tides of the British Channel are pretty well known; and therefore it would be possible by very laborious quadratures to determine the deflection of the plumb-line due to the attraction of the tide at any time at a chosen station. If, then, the deflection of the plumb-line could be observed at that station (with corrections applied for the positions of the sun and moon), the ratio of the calculated to the observed and corrected deflection, together with the known value of the earth's radius and mean density, form the materials for computing the rigidity. But such a scheme would be probably rendered abortive by just such comparatively large and capricious oscillations of the vertical as we, M. d'Abbadie, and others have observed.

It is interesting to draw attention to some observations of M. d'Abbadie on the deflections of the vertical due to tides. His observatory (of which an account was given in the Report for 1881) is near Hendaye, in the Pyrenees, and stands 72 metres above and 400 metres distant from the sea. He writes*:—

"J'ai réuni 359 comparaisons d'observations spéciales faites lors du maximum du flot et du jusant ; 243 seulement sont favorables à la théorie de l'attraction exercée par la masse des eaux, et l'ensemble des résultats pour une différence moyenne de marées égale à 2·9 mètres donne un résultat moyen de 0″·56 ou 0″·18 pour le double de l'attraction angulaire vers le Nord-Ouest. Ceci est conforme à la théorie, car les différences observées doivent être partagées par moitié, selon la loi de la réflexion ; mais comme il y a toujours de l'inattendu dans les expériences nouvelles, on doit ajouter que sur les 116 comparaisons restantes il y en a eu 57 où le flot semble repousser le mercure au lieu de l'attirer. Mes résultats ont été confirmés pendant l'hiver dernier par M. l'abbé Artus, qui a eu la patience de comparer ainsi 71 flots et 73 jusants consécutifs, de janvier à mars 1880. Lui aussi a trouvé un tiers environ

* "Recherches sur la Verticale," *Ann. de la Soc. Scient. de Bruxelles,* 1881.

de cas défavorables à nos théories admises. On est donc en droit d'affirmer que si la mer haute attire le plus souvent le pied du fil à plomb, il y a une, et peut-être plusieurs, autres forces en jeu pour faire varier sa position."

We must now consider the vertical displacement of the land near the coast. In (21) it is shown to be

$$\alpha_0 = \frac{gwh}{\pi v} \times \frac{l}{\pi^2} \times 2{\cdot}1037,$$

where α_0 indicates the displacement corresponding to $z=0$.

With the assumed values, $h=40$, $v=3\times 10^8$, $l=6{\cdot}28\times 10^8$, I find $\alpha_0=5{\cdot}684$ centim. Hence the amplitude of vertical displacement is $11{\cdot}37$ centim. As long as hl remains constant this vertical displacement remains the same; hence the high tides of 10 or 15 feet which are actually observed on the coasts of narrow seas must probably produce vertical oscillations of quite the same order as that computed.

If the land falls, the tide of course rises higher on the coast-line than it would do otherwise; hence the apparent height of tide would be $h+\alpha_0$. But this shows there is more water resting on the earth than according to the estimated value h; hence the depression of the soil is greater in the proportion $1+\alpha_0/h$ to unity; this again causes more tide, which reacts and causes more depression, and so on. Thus on the whole the augmentation of tide due to elastic yielding is in the ratio of

$$1+\frac{\alpha_0}{h}+\left(\frac{\alpha_0}{h}\right)^2+\left(\frac{\alpha_0}{h}\right)^3+\ldots \text{ or } \frac{1}{1-\alpha_0/h} \text{ to unity.}$$

This investigation is conducted on the equilibrium theory; and it neglects the curvature of the sea-bed, assuming that there is a uniform slope from mid-ocean to the sea-coast. The figure shows that this is not rigorously the case; but it is quite near enough for a rough approximation. The phenomena of the short-period tides are so essentially kinetic that the value of this augmentation must remain quite uncertain; but for the long-period tides (the fortnightly and monthly elliptic) the augmentation must correspond approximately with the ratio

$$1 : \left(1-\frac{gwl}{\pi^3 v}\times 2{\cdot}1037\right).$$

The augmentation in narrow seas will be small; but in the Atlantic Ocean the augmenting factor must agree pretty well with that which I now compute*.

* Sir William Thomson has pointed out to me, since the meeting of the Association, that this augmentation will only hold true in the cases of certain distributions of land.

With the previous numerical values we have α_0/h (which is independent of h) equal to ·1421, and $1-\alpha_0/h = \cdot 8579 = \frac{6}{7}$ very nearly.

Thus the long-period tides may probably undergo an augmentation at the coasts of the Atlantic in some such ratio as 6 to 7.

The influence of this kind of elastic yielding is antagonistic to that reduction of apparent tide which must result from an elastic yielding of the earth's mass as a whole.

The reader will probably find it difficult to estimate what degree of probability of correctness there is in the conjectural value of the rigidity, which has been used in making the numerical calculations in this paper. The rigidity has not been experimentally determined for many substances; but a great number of experiments have been made to find Young's modulus. Now, in the stretching of a bar or wire the compressibility plays a much less important part than the rigidity; and the formula for Young's modulus shows that for an incompressible elastic solid the modulus is equal to three times the rigidity*. Hence a third of Young's modulus will form a good standard of comparison with the assumed rigidity, namely 3×10^8 grammes weight per square centimetre. The following are a few values of a third of Young's modulus and of rigidity, taken from the tables in Sir William Thomson's article on Elasticity† in the *Encyclopedia Britannica*:—

Material.	A third of Young's modulus and rigidity in terms of 10^8 grammes weight per square centimetre.
Stone	About 1·2
Slate	About 3 to 4.
Glass	Rigidity 1·5 to 2·4.
Ice	4·7.
Copper	4, and rigidity 4·6 to 5·4.
Steel	7 to 10, and rigidity 8·4.

It will be observed that the assumed rigidity 3 is probably a pretty high estimate in comparison with that of the materials of which we know the superficial strata to be formed.

It is shown, in another paper read before the Association at this meeting, that the rigidity of the earth as a whole is probably as great as that of steel. That result is not at all inconsistent with the probability of the assumption that the upper strata have only a rigidity a little greater than that of glass.

* Thomson and Tait's 'Natural Philosophy,' § 683.

† Also published separately by Black (Edinburgh).

3. *On Gravitational Observatories.*

In the preceding sections estimates have been made of the amount of distortion which the upper strata of the earth probably undergo from the shifting weights corresponding to barometric and tidal oscillations. These results appear to me to have an important bearing on the utility of gravitational observatories.

It is not probable, at least for many years to come, that the state of tidal and barometric pressure, for a radius of 500 miles round any spot on the earth's surface, will be known with sufficient accuracy to make even a rough approximation to the slope of the surface a possibility. And were these data known, the heterogeneity of geological strata would form a serious obstacle to the possibility of carrying out such a computation. It would do little in relieving us from these difficulties to place the observatory at the bottom of a mine.

Accordingly the prospect of determining experimentally the lunar disturbance of gravity appears exceedingly remote; and I am compelled reluctantly to conclude that continuous observations with gravitational instruments of very great delicacy are not likely to lead to results of any great interest. It appears likely that such an instrument, even in the most favourable site, would record incessant variations of which no satisfactory account could be given. Although I do not regard it as probable that such a delicate instrument should be adopted for regular continuous observations, yet, by choosing a site where the flexure of the earth's surface is likely to be great, it is conceivable that a rough estimate might be made of the average modulus of elasticity of the upper strata of the earth for one or two hundred miles from the surface.

These conclusions, which I express with much diffidence, are by no means adverse to the utility of a coarser gravitational instrument, capable, let us say, of recording variations of level amounting to $1''$ or $2''$. If barometric pressure, tidal pressure, and the direct action of the sun and moon combine together to make apparent slope in one direction, then, at an observatory remote from the sea-shore, that slope might perhaps amount to a quarter of a second of arc. Such a disturbance of level would not be important compared with the minimum deviations which could be recorded by the supposed instrument.

It would then be of much value to obtain continuous systematic observations, after the manner of the Italians, of the seismic and slower quasi-seismic variations of level.

I venture to predict that at some future time practical astronomers will no longer be content to eliminate variations of level merely by taking means of results, but will regard corrections derived from a special instrument as necessary to each astronomical observation.

XLVII. *New Views of* Mr. George H. Darwin's *Theory of the Evolution of the Earth-Moon System, considered as to its bearing on the question of the Duration of Geological Time.* *By the* Rev. Samuel Haughton, *M.D., Fellow of Trinity College, Dublin**.

It has been tacitly assumed, even so far back as the times of Newton and Clairaut, that the earth and planets have passed through a liquid condition (owing to former great heat) before assuming the solid condition which some, at least, of them now possess.

Laplace, in his nebular hypothesis, also assumes the former existence of this liquid condition; and it is openly asserted by all geologists who believe that the earth consists of a solid crust (more or less thick), reposing upon a fluid or viscous nucleus.

It has been proved by Sir William Thomson, following out the views of the late Mr. Hopkins, that the present condition of the earth, taken as a whole, is such that it must be regarded as being more rigid than glass or steel, possibly more rigid than any terrestrial substance under the surface-conditions of pressure.

The following considerations show that it may be fairly doubted whether the earth or any other planet ever existed in a fluid condition.

1. The possibility of the equilibrium of the rings of Saturn, on the supposition that they are either solid or liquid, has been more than doubted, and the most probable hypothesis respecting them is, that they consist of swarms of discrete meteoric stones.

2. It is difficult to understand the low specific gravity of Jupiter and the other outer planets, on the supposition that they are either solid or liquid; for we know of no substance light enough to form them †. If the outer planets consist of

* From the 'American Journal of Science' for November 1882. Read before the Mathematical Section of the American Association for the Advancement of Science, at Montreal, August 1882.

† The force of this argument could not be felt before the revelations of the spectroscope, because at that time there was no proof that the whole universe was composed of the same simple substances, and those very limited in number.

discrete meteoric stones moving around a solid or liquid nucleus, the difficulty respecting their specifie gravity would disappear.

3. The recent researches connecting the November, the August, and other periodic swarms of shooting-stars with comets, tend in the direction of showing that comets in cooling break up into discrete solid particles (each no doubt having passed through the liquid condition), and that probably the solar nebula cooled in like manner into separate fiery tears, which soon solidified by radiation into the cold of space.

4. Mr. Huggins's recent comparisons of the spectroscopic appearances of comets and incandescent portions of meteoric stones, showing the presence in both of hydrocarbon and nitrogen compounds, confirm the conclusions drawn from the identity of the paths of comets and meteoric periodic shooting-stars.

5. Mr. H. A. Newton, in a remarkable paper read before the Sheffield Meeting of the British Association (1879), showed the possibility (if not probability) of the asteroids being extinct comets, captured and brought into the solar system by the attraction of some one or other of the outer large planets, and permanently confined in the space between Mars and Jupiter, which is the only prison-cell in the solar system large enough to hold permanently such disorderly wanderers.

In the same paper Professor Newton threw out the idea that some of the satellites of the large planets might also be of cometary origin.

From all these and other considerations it is therefore allowable to suppose that the earth and moon, when they separated from the solar nebula, did so as a swarm of solid meteoric stones, each of them having the temperature of interstellar space, *i. e.* something not much warmer than 460° F. below the freezing-point of water.

Mr. George H. Darwin has shown admirably how the earth-moon system may have been developed from the time when the earth-moon formed one planet, revolving on its axis in a few hours, to the present time, when the earth and moon (in consequence of tidal friction) have pushed each other asunder to a distance of sixty times the radius of the earth *.

In his paper on the tidal friction of a planet† (supposed viscous and under the influence of bodily tides caused in it by an external body such as the moon), Mr. Darwin has found a remarkable equation of condition, which may be thus

* Proceedings of the Royal Society, 19th June, 1879.

† Phil. Trans. 1881, part ii. p. 494.

expressed :—

$$d(\sqrt{r}) \propto \frac{\Psi dt}{r^6}, \quad \ldots \ldots \quad (1)$$

where

r = distance between centres of earth and moon,
t = time elapsed from a fixed point,

$$\Psi = \frac{p(n-\Omega)}{1+p^2(n-\Omega)^2}, \quad \ldots \ldots \quad (2)$$

n = angular velocity of earth's rotation,
Ω = angular velocity of moon's orbital revolution,
p = quantity varying inversely as the viscosity of the planet.

The extreme interest of equation (1) consists in the appearance of the inverse sixth power of the distance.

As the function Ψ varies very slowly, we find by integration, for any portion of time during which Ψ may be regarded as constant,

$$t = \mathrm{A}r^{\frac{13}{2}} + \mathrm{B}, \quad \ldots \ldots \quad (3)$$

a most unexpected and remarkable result.

Upon reading Mr. Darwin's papers, my mind turned to a problem with which I was familiar, viz. the retardation of the earth's rotation produced by the lunisolar tide exerted upon the ocean supposed collected in an equatorial canal, the moon and sun having no declination; and I readily found an equation to express the evolution of the earth-moon system, on the foregoing hypothesis as to friction.

This equation is the following :—

$$d(\sqrt{r}) \propto \frac{f\Phi dt}{r^6}, \quad \ldots \ldots \quad (4)$$

where

$$\Phi = \frac{\mathrm{V}_0(n-\Omega)}{\{4\mathrm{V}_0^2(n-\Omega)^2 - k^2\}\sqrt{4(n-\Omega)^2+f^2}}, \quad \ldots \ldots \quad (5)$$

f = coefficient of friction supposed proportional to relative velocity,
k varies inversely as r^3,
V_0 = velocity at earth's equator.

This leads, as in Mr. Darwin's hypothesis of viscous earth, to the integral

$$t = \mathrm{A}'r^{\frac{13}{2}} + \mathrm{B}'. \quad \ldots \ldots \quad (6)$$

The form of the functions Ψ and Φ is similar, as both ascend by odd powers of $(n-\Omega)$ and vanish when $n=\Omega$—that is to say, at the beginning and end of the evolution by friction of the earth-moon system.

It is quite clear, therefore, that the remarkable expression (1) found by Mr. Darwin is not peculiar to his special hypothesis of a viscous earth, but can be deduced equally well from the totally distinct hypothesis of an absolutely rigid earth retarded by the tidal action of a liquid ocean.

I was led by this result to consider the case of the earth-moon, separating (as I believe they did) from the central solar mass in the form of a swarm of discrete masses of meteoric iron and stone, each one having the temperature of the cold of interstellar space, or not much above it. Translating this conception into mathematical language, I find that the equation of continuity belonging to the hydrodynamical theory applies equally well to the meteoric theory, viz.

$$vy=v'y', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where v, v' are the velocities at any two points, and y, y' are the depths of the ocean or meteoric swarm at the same points.

The depth of the swarm or ocean without jostling or friction will be least under the moon and greatest at right angles to the moon, and the velocities will be inversely. Hence the chances of jostling among the meteorites when disturbed by the moon's tidal action will be proportional to the velocity, being greatest where the velocity is greatest and the area of passage least, and *vice versâ*.

This consideration reduces the meteoric problem to that of the hydrodynamical problem, with a friction proportional to the velocity, and gives equations in all respects similar to those derived by Mr. Darwin from the hypothesis of a viscous earth.

On the meteoric hypothesis, if the jostling of the stones be slow they may cool almost as fast as they are heated, and the result will be a cool earth and almost indefinite time at the disposal of geologists.

XLVIII. *On Systems of Absolute Measures for Electric and Magnetic Quantities.* *By* Prof. H. Helmholtz*.

PHYSICISTS have hitherto been obliged to employ two different systems of electrical absolute measures, the *electrostatic* and *electromagnetic*; while for *magnetic quantities* only one has always been made use of—namely that introduced by Gauss, in which only the parts of the metre and the gramme employed as the units of length and mass have changed. Indeed the employment of those two systems of electrical measures could not be dispensed with, for practical reasons, because the determination of the factor which had to be used

* Translated from Wiedemann's *Annalen*, 1882, no. 9, vol. xvii. pp. 42–54.

for the reduction of electrostatic to electromagnetic measures, namely Weber's critical velocity, could not yet be effected with the same degree of precision that could be attained within the sphere of electromagnetic measurements on the one hand and electrostatic measurements on the other. It was on this account more advantageous to employ in each experimental investigation that system of measures to which the quantities measured could be referred with the greater exactness.

To this is to be added the consideration of avoiding excessively large numbers, which will probably induce us to continue to employ for electrostatic and galvanic phenomena two kinds of measures, although reducible to one another. At present the electromagnetic methods of measurement are the most perfect; they are unmistakably the most important practically for an art that advances with giant strides; and I have therefore considered that the International Congress that met in Paris last year acted quite suitably in endeavouring to establish an electromagnetic system of absolute measures. Had the aim been purely scientific, I should have preferred the electrostatic system hitherto employed, since this, I think, best represents the essential analogies of the phenomena by analogous formulæ, and gives to them the clearest and most intelligible expression. It was on this system, grounded on Gauss's principles, that most of the physical-mathematical treatises in this department of science have hitherto been based.

Just on this account it would appear to me very undesirable if this system should now entirely fall, and even its name give place to a new one, as proposed by Clausius in his recently published memoir*. I would not at all recommend the multiplication of systems of measures without very urgent reasons; and certainly the transference of a name already in use and frequently employed to a new system would inevitably produce needless and vexatious confusion in physical literature, even apart from any estimate of the relinquished in comparison with the new system.

Any determination of a new absolute measure must be based on the measuring observation of a natural process or behaviour, just as already, among the three fundamental units, the gramme has been reduced to the two others by means of the density of pure water at 4° C. The measure of magnetic quanta which has hitherto been exclusively employed is founded on the definition laid down by Gauss, according to which the repellent force between two magnetic quanta, m_1 and m_2, which are situated at the distance r from one another,

* Clausius, *Verhandl. des naturh. Vereins d. preuss. Rheinl. u. Westfal.* March 6, 1882; Wied. *Ann.* xvi. p. 529; Phil. Mag. June 1882, xiii. p. 381.

is put not merely proportional, but equal to the value of $(m_1 . m_2/r^2)$. Since the force and the length r are to be measured by known methods, the value of the product $(m_1 . m_2)$ is thereby determined in absolute measure; and therefore, if from other facts the ratio (m_1/m_2) can be determined, m_1 and m_2 can each be separately determined.

Exactly the same principle is also applied by Gauss, at the commencement of his memoir "Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Anziehungs- und Abstossungskräfte"*, to electrical quanta and gravitating masses. Although he has not in the latter two cases carried the principle into practical effect, it would be justifiable to designate all three methods by his name as that of their mental author. That which refers to electricity gives the *electrostatic system* as it has hitherto been employed. The third, referring to gravitating masses, will probably in future play an important part, when we have succeeded in accomplishing more exact determinations of the force of gravitation. If, like Maxwell, we denote by angular brackets the dimensions of the expression enclosed in them, by M a mass, by L a length, and by T a time, according to Gauss the attraction between two heavy masses m at the distance r is

$$\left[\frac{m^2}{r^2}\right]=\left[\frac{\mathrm{M.L}}{\mathrm{T.T}}\right] \text{ or } \left[\frac{\mathrm{M}}{\mathrm{L}^3}\right]=\left[\frac{1}{\mathrm{T}^2}\right].$$

On the left stands a density, on the right a function of the time. If, therefore, as hitherto, we put the absolute density of water equal to *unity*, while the unit of mass is determined in gravitation-measure, a time-measure is thereby given which is independent of the probably variable rotation of the earth, and only a single measure, the metre, is left to be handed down by tradition. But even this could be absolutely defined if we availed ourselves of an invariable velocity, for instance the velocity of light in free æther.

Thus, for example, the period of revolution T of a small satellite revolving close to the surface of a sphere of pure water of normal density D, would, independently of the radius of the sphere, in gravitation-measure be

$$\mathrm{T}^2=\frac{3\pi}{\mathrm{D}},$$

and the velocity of light

$$v=\frac{\mathrm{L}}{\mathrm{T}}=\mathrm{L}\sqrt{\frac{\mathrm{D}}{3\pi}},$$

* *Resultate aus den Beobachtungen des magnetischen Vereins* 1839.

by which latter equation the length L would be given. This system would therefore free us from the handing-down of any traditional measure.

In Gauss's magnetic and electrostatic measure the dimensions of the magnetic quantum m and the electrostatic quantum e are determined by the equations

$$[m]=[e]=[M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}],$$

both based on the phenomenon of repulsion between resting magnetic or resting electric quanta.

On the other hand, for electromagnetic determinations the ponderomotive action of a closed electric current upon a pole of a magnet was used, the laws of which have been mainly and completely formulated by Ampère.

The components of the magnetic forces produced in its vicinity by an electric current can, like those of a magnet, be represented as differential quotients of a potential-function which satisfies the same differential equations as those of magnets, and only differs from those of the latter in that it periodically increases in value by the same quantity as often as only one pole is caused to make a whole revolution about the conductor of the current. As the electromagnetic forces are proportional to the current-intensity of the conductor, the period of the potential is also proportional to that intensity, and independent of the shape of the conductor. Maxwell on this account employs the value of the period of the potential Ω as a measure for the intensity of the current C, and therefore, in § 623 of his Treatise on Electricity and Magnetism, puts the dimensions of the two equal:

$$[\Omega]=[C].$$

The fixed numerical relation between the two follows from an earlier passage of the above-mentioned work, § 479, where T denotes the magnetic force of a very long straight current-conductor at the small distance r from its axis, and J is put for C:—

$$Tr=2J.$$

Since

$$\Omega=T \, . \, 2\pi r,$$

$$\Omega=4\pi J,$$

by which Maxwell's determination becomes, when Gauss's magnetic measure is employed, equal to the electromagnetic measure proposed by W. Weber.

In Ampère's time a complete theory of potential-functions did not yet exist. He has, however, represented quite accu-

rately what we can now, in the manner stated, express in conformity with nature, by a suitably chosen fiction; namely, he imagined a surface bounded by the conductor, dividing the in this case doubly connected space covered with a double magnetic layer. If the magnetic moment of each unit of surface of the double layer is denoted by μ, according to well-known principles the leap of potential between the two sides must be

$$\Omega = 4\pi\mu,$$

and therefore $\mu = J$. With this form of expression of Ampère's law Prof. Clausius stops.

Both forms are perfectly equivalent and equally justified, so long as we measure the magnetic quanta according to Gauss's rule. This Prof. Clausius also admits; but he thinks an extension of Maxwell's expression to other systems of measures must be rejected; he explains this as an oversight on Maxwell's part, and the equations and determinations of measurements derived from it as erroneous.

The only reason which, in this respect, he alleges against Maxwell's definition is the following, in § 3 of the memoir above cited:—"The force, however, which a current exerts upon a magnetic pole is *electrodynamic*; and from this it follows that an equation of which the deduction is based upon this force can be regarded as valid only in the dynamic system founded upon the electrodynamic forces, and not in the static system based on the electrostatic forces."

But even if one, as an adherent of Ampère's hypothesis, entertained no doubt respecting the first part of this proposition, I do not see why the conclusion should be urged against Maxwell only, and not against the interpretation of Ampère's law adopted by Clausius himself, since the latter is, after all, nothing but another way of expressing the same facts. Both quantities, Maxwell's potential-period Ω as well as Ampère's magnetic momentum of unit surface, are, in Gauss's system of measurement, of the dimension $[m/L]$; both have a physical meaning only in "the representation of the force which a current exerts upon a magnetic pole."

The true reason of the difference moreover appears to me to lie in quite another circumstance—namely, in Maxwell's definition of the magnetic potential Ω. This is with him not the form of calculation $\Sigma[m/r]$, but he defines it in this case, as also in the corresponding applications to electrostatics and electrodynamics, by stating that $\Omega \,.\, m$ is a work—which definition also underlies Gauss's definition of m.

The differential quotient $-d\Omega/dx$ is, corresponding to this, the force which acts upon the unit of magnetism, and therefore

$(-\Omega)$ Jacoby's force-function. If we introduce for m another measuring unit, and measuring with it obtain m_1, Ω_1, J_1 instead of m, Ω, J, then must, according to Maxwell's definition,

$$m\Omega = m_1\Omega_1,$$

and therefore

$$mJ = m_1J_1.$$

The unit of current therefore increases in the inverse proportion of the newly chosen unit of magnetism to the old one; but the force which the unit of current exerts upon the unit of magnetism remains constant. With Prof. Clausius, on the contrary, both m and J increase in the same, and not in the inverse ratio, and the force increases as m^2 or J^2. According to his determinations, the feigned surface of Ampère always produces one more leap, equal to the momentum of unit of surface, in the function $\Sigma(m/r)$; but this function has then no longer the signification of the force-function for the newly chosen magnetic units.

In all this I cannot perceive any mistake of Maxwell's; and his equations, derived from the formulation chosen by him of the fundamental phenomenon, are altogether as consistent with each other and as correct, if understood in the sense of their author, as those of Prof. Clausius. Rather, this case shows that, if we abandon Gauss's determination of the magnetic unit, we again fall into at least two different and equally justifiable systems of measuring-units; and, for my part, I could, with respect to both Maxwell and Clausius, draw from this the practical conclusion that we ought by no means to forsake the above-mentioned method of Gauss until we have particularly important reasons and a definite purpose for such a proceeding, when the choice between the systems of Maxwell and Clausius would probably be decided on positive grounds.

Now, although I must vindicate Maxwell from the charge of having, in consequence of an oversight, set up incorrect equations, yet it should be mentioned that in the wording of the text of § 623 of his work, where he reduces the dimensions of all the rest of the electric and magnetic quantities to the dimensions of any one chosen from among them, an omission occurs which might easily lead the reader into error, if he does not closely examine the connexion of the somewhat complicated systems of equations of §§ 622 and 623, and which seems to give to the propositions of § 623 a greater extension than Maxwell himself would probably have given them. For at the beginning of Chapter X., in § 620, he speaks of electrostatic and electromagnetic but not of magnetic units, and

moreover gives definitions according to which the electrokinetic units are determined without any consideration of magnetism. But in § 621 he introduces magnetic quantities without in any way saying expressly that, in all his determinations of the ratio between electric and magnetic quantities, the electromagnetic determination discussed in the chapters in question,

$$4\pi J = \Omega,$$

will be retained.

In this respect Maxwell's intention in sketching different systems was exactly the same, and as limited, as that more recently carried out by Clausius, although, from the cause above discussed, the manner of carrying it out by the two has turned out different.

The matter being so important, I will take leave to give here a summary of the connexion of the equations of the corresponding paragraphs (621–623) of Maxwell's Treatise on Electricity and Magnetism. In § 621 he arranges the quantities for which the dimensions of the unit are to be determined. I place them here in a somewhat different order, and with the notation of their meaning which is more usual in Germany. There are four electrostatic, namely:—

e, electric quantum;

E, electrostatic potential-function;

$\mathfrak{D}$, dielectric polarization, measured by the electric density at the surface of the dielectric;

$\mathfrak{E}$, the electric force in a point, acting upon the unit of s.

The four corresponding magnetic quantities he denotes, in the same order, by m, Ω, $\mathfrak{B}$, $\mathfrak{H}$.

To these are added four more corresponding quantities, namely:—

C, the intensity of a current;

$\mathfrak{C}$, current-density;

p, the electrokinetic momentum of a current;

$\mathfrak{A}$, the vector-potential of electric currents.

As regards the meaning of the last two quantities, p is Neumann's electrodynamic potential of the other currents present, referred to the entire conductor (passed through by the unit of current) for which it is calculated; and $\mathfrak{A} . ds$ is the same potential referred to a conductor-element ds situated at a determined place.

I remark further that the quantities denoted by German capital letters are vectors, *i. e.* quantities having direction and resolvable into components according to the law of the parallelogram of forces,—and that the selection of them resulted from Maxwell's endeavour to introduce, as far as possible, only

directly observable quantities into the calculation, and to avoid hypotheses.

In § 622 Maxwell sets forth dimension-determinations for the products and quotients of these twelve quantities, as given immediately by their meaning. There are fifteen of these determinations, in which those quantities referable to electricity are kept quite separate from those which refer to magnetism, so that (which is certainly remarkable) from these fifteen determinations no relation between electricity and magnetism appears. I arrange them tabularly, retaining Maxwell's above-employed notation of the dimensions.

No.	Dimension.	Electric quantities.	Magnetic quantities.
1.	L^2M/T^2	$[e.\mathrm{E}]$, $[p.\mathrm{C}]$	$[m.\Omega]$
2.	$M/(LT^2)$	$[\mathfrak{D}.\mathfrak{E}]$, $[\mathrm{C}.\mathfrak{A}]$	$[\mathfrak{B}.\mathfrak{H}]$
3.	T	$[e/\mathrm{C}]$, $[p/\mathrm{E}]$, $[\mathfrak{A}/\mathfrak{E}]$	
4.	L	$[\mathrm{E}/\mathfrak{E}]$, $[p/\mathfrak{A}]$	$[\Omega/\mathfrak{H}]$
5.	L^2	$[e/\mathfrak{D}]$, $[\mathrm{C}/\mathfrak{C}]$	$[m/\mathfrak{B}]$

The last series of determinations result from the first, if they be divided by the product of the quantities in question of the second and fourth series. The fifth series may therefore be omitted as superfluous; then there remain the following identical equations between the quantities in the first four rows:—

$$\left[\frac{p}{\mathrm{E}}\right]=\frac{[p.\mathrm{C}]}{[e.\mathrm{E}]}\cdot\left[\frac{e}{\mathrm{C}}\right];$$

$$\left[\frac{\mathfrak{A}}{\mathfrak{E}}\right]=\frac{[p.\mathrm{C}]\cdot\left[\frac{e}{\mathrm{C}}\right]}{[e.\mathrm{E}]\cdot\left[\frac{p}{\mathfrak{A}}\right]}\cdot\left[\frac{\mathrm{E}}{\mathfrak{E}}\right].$$

Lastly, there are left three independent determinations for the four magnetic quantities m, Ω, $\mathfrak{B}$, $\mathfrak{H}$, and seven for the eight electric quantities e, E, p, C, $\mathfrak{D}$, $\mathfrak{E}$, $\mathfrak{C}$, $\mathfrak{A}$. If, therefore, of these two groups one quantity each be defined by other determinations—for example m and e, the quantum of magnetism and the quantum of electricity—all the others are completely defined, namely:—

	Electric.	Electrokinetic.	Magnetic.
Potentials ...	$[\mathrm{E}]=\left[\frac{\mathrm{L}^2\mathrm{M}}{e\mathrm{T}^2}\right]$	$[p]=\left[\frac{\mathrm{L}^2\mathrm{M}}{\mathrm{CT}^2}\right]$	$[\Omega]=\left[\frac{\mathrm{L}^2\mathrm{M}}{m\mathrm{T}^2}\right]$
Forces	$[\mathfrak{E}]=\left[\frac{\mathrm{LM}}{e\mathrm{T}^2}\right]$	$[\mathfrak{A}]=\left[\frac{\mathrm{LM}}{\mathrm{CT}^2}\right]$	$[\mathfrak{H}]=\left[\frac{\mathrm{LM}}{m\mathrm{T}^2}\right]$
Densities ...	$[\mathfrak{D}]=\left[\frac{e}{\mathrm{L}^2}\right]$	$[\mathfrak{C}]=\left[\frac{\mathrm{C}}{\mathrm{L}^2}\right]$	$[\mathfrak{B}]=\left[\frac{m}{\mathrm{L}^2}\right]$
$[e]=[\mathrm{CT}]$			

These are the determinations which, without any further limiting equations, result from the above fifteen. These can be applied to any definition of the units of m and e, and therefore also to the *electrostatic-magnetic* system of Gauss.

Now follows, in Maxwell, § 623, "These fifteen equations are not independent of each other; and in order to deduce from them the dimensions of all the twelve units they contain, we require one more equation." In fact we require two, since e and m must be determined singly by recurring to two facts of observation regarded as fundamental phenomena. The one here wanting, not expressly mentioned by Maxwell, but from the connexion self-evidently presupposed, we can write as above:—

$$[\Omega]=[\mathrm{C}].$$

Clausius has chosen for it the less perspicuous

$$[p]=[m].$$

But, since one of the fifteen determinations in § 622 reads

$$[m\,.\,\Omega]=[p\,.\,\mathrm{C}],$$

each of the two follows from the other.

Just on this account, however, the closing words of § 623, "All the above-given determinations are correct for any system of units we may choose," must be altered, and limited to electromagnetic systems, and, indeed, to such only as are derived from the meaning, as defined by Maxwell, of the fundamental law of electromagnetism. For that coucluding sentence applies neither to the electrostatic system nor to the system set up by Clausius. Of the possibility that another conception of the electromagnetic fundamental law might here lead to other consequences Maxwell probably did not think; and in this respect Clausius has indeed, in his most recent memoir, given a thankworthy enrichment of our ideas.

Finally, we must speak of the reason why Prof. Clausius is

willing to drop the electrostatic system, constructed according to Gauss's principles, hitherto employed. The only thing he says on this subject is contained in § 1 of his memoir. After mentioning that the forces exerted upon each other by closed electric currents may be regarded as indubitably known, he continues:—"As, further, the small electric currents which according to Ampère are to be assumed as existing in the interior of a magnet are likewise closed, we have in magnetism to do with forces of the same kind." Thereupon follows an analysis, according to which the forces of two magnetic quanta are to be regarded as dynamic.

This reason, however, would be decisive only if it were certainly proved that Ampère's hypothesis corresponds to the reality, while up to the present it can hardly be said that it has been clearly and consistently worked out for all sorts of diamagnetic and ferromagnetic bodies. In particular, this hypothesis would also require that the hypothetic molecular currents of magnetized bodies should show the changes which are to be generated by electrodynamic induction, as they are in fact logically assumed to do by W. Weber in his well-known explanation of diamagnetic polarization. How this can be reconciled with the properties of ferromagnetic bodies I leave the adherents of this theory to explain. Meanwhile, however interesting this theory may be, we may look upon it as neither verified nor even completely worked out.

Among the electrodynamic theories which assume direct action at a distance, its quantity and direction depending on the absolute or relative motions of two electric quanta, stands that of Faraday and Maxwell. It has at least this superiority to those, that it does not violate either the principle of the finiteness and constancy of energy or that of the equality of action and reaction; and moreover it bases a theory of light, free from many difficulties of the hitherto received undulation theory, upon the identical hypotheses which form the groundwork of electrodynamics. In order to discover the essential character of the forces of electricity and magnetism, Maxwell excludes those processes in which, according to the sort of friction, heat is generated and electric or magnetic energy lost, and founds his theory upon conservative processes only. In particular, he excludes the conduction of electricity in conductors, and the coercive force in magnets. Then, however, his fundamental equations present the most complete analogy, not between magnets and moved electricity, but between resting dielectric and resting magnetic polarization. It is precisely to this analogy that Gauss's electrostatic system of measures perfectly accommodates itself.

I will make one more remark, that when one seeks to form, after the analogy of Hamilton's principle, that integral, taken with respect to the time, whose variation gives the equations of motion according to Maxwell in place of the *electrodynamic potential* of Clausius (Maxwell's *electrokinetic energy*), a bilinear function of the components of the electric flow, on the one hand, and of the components of the magnetic momenta, on the other, arises in which the latter have to be dealt with, but *not* as velocities. This last point I reserve to myself to treat soon in another place.

For the present I need only remind physicists that the ground on which Prof. Clausius is inclined to reject the hitherto accepted electrostatic system is a hypothetical assumption, contested between different theories; and I would beg them not to transfer the name of the *electrostatic* system proceeding from Gauss, and hitherto employed by preference in mathematical works, to another. In this system the potentials (m^2/r) and (e^2/r) are quantities of work; their entire physical importance rests upon the fact that they are such. The theory of the potential-functions forms one of the most complex and interesting chapters of mathematical physics, corresponding to well and perfectly known physical processes. If Gauss's units be changed, then must we accustom ourselves to add factors to all potential-functions, in order that they may remain quantities of work and their differential quotients give the forces. On the contrary, whether J^2 is a force and mJ a work, or whether we must write for them A^2J^2 and AmJ, appears to me much less important, especially as we know well and accurately just a portion of the department of electromagnetic actions, viz. that consisting of closed currents, but in the province of unclosed currents the most luxuriant flora of hypotheses still flourishes.

XLIX. *The Thomson Effect.* *By* JOHN TROWBRIDGE *and* CHARLES BINGHAM PENROSE*.

SIR WILLIAM THOMSON† first discovered that when an electrical current passes through a piece of metal, the ends of which are of different temperatures, it carries heat with it; the direction depending upon the character of the metal and the direction of the current. This phenomenon is known as the Thomson Effect. Le Roux‡ subsequently verified Thomson's results, and gave an incomplete table of the effect in different metals. No especial pains have

* From Silliman's American Journal of Science for November 1882.

† Phil. Trans. 1856, vol. iii. p. 661.

‡ *Ann. de Chim. et de Phys.* 1867, [4] vol. x. p. 258.

been taken hitherto in experimenting with pure metals. We have thought it would be valuable to test the effect in as pure a metal as we could obtain by electrolysis. We have also extended Le Roux's table by the addition of the effect in nickel, which Thomson was unable to obtain, and also in carbon. An endeavour has been made to ascertain if the effect is reversible, and also to discover if it is modified in a magnetic field.

The strip of nickel, 45 centim. long, 2·6 wide, and 2 millim. in thickness, was placed with its flat surface horizontal. One face of a thermopile was placed at a fixed point on the surface of the nickel, separated from it by a thin piece of mica. A weight pressed upon the other surface. The thermopile was connected with a Thomson's reflecting galvanometer of six ohms resistance. The two extremities of the strip of nickel were connected with a battery of six Grove cells, the wires first passing through a key so that the direction of the current could be reversed. One end of the nickel was kept at the temperature of the air, 15° C.; the other at a constant red heat by means of a Bunsen burner. The metal was heated in this way from 9 A.M. to 3 P.M., until it reached a condition of thermal equilibrium, as shown by the galvanometer. The scale of the galvanometer was then moved until the spot of light came to 0. The current from the Grove cells was then passed for one minute alternately in opposite directions, and the deflections of the galvanometer were read every quarter of a minute. Before the direction of the current was changed, the circuit was each time broken, and the spot of light was allowed to fall to 0. The following table gives the results. The column marked "C–H" gives the deflections when the current was passing from cold to hot. The small numbers show which deflections in each pair were taken first.

C–H. Deflections taken every ¼ minute.					H–C. Deflections taken every ¼ minute.				
1	2	1	2	1	2	1	2	1	2
4·1	4·2	4·0	4·3	4·4	3·3	4·0	3·6	3·8	4·1
6·3	6·4	6·5	6·4	6·4	5·0	6·2	5·4	5·9	6·0
7·3	7·2	7·5	7·0	7·2	5·8	6·7	6·2	6·5	7·0
7·4	7·6	7·6	7·3	7·7	6·1	7·3	6·5	6·8	7·2

From this table it is obvious that more heat is evolved by a constant current per unit time in passing from the cold to the hot end of the nickel than in passing in the opposite direction. The Thomson Effect in pure nickel is consequently negative;

i. e. heat is absorbed by a current in passing from hot to cold, and evolved in passing from cold to hot. The above results were confirmed by many similar experiments, as will be seen later.

It was next determined to find whether the Thomson Effect was reversible—that is, whether the heat absorbed by a current in passing across a section of temperature t was equal to the heat evolved by the same current when passing in the opposite direction across the same section. This subject has important bearings on the thermodynamical theory of thermoelectricity.

The following method was pursued:—Both ends of the nickel were at the temperature of the air, 15° C. The current from six Grove cells was passed as before, and the deflections of the galvanometer were observed every half-minute. The apparatus was arranged exactly as before. Column I. of the accompanying table gives the deflections. One end of the nickel was now placed in melting ice. After one hour it reached a condition of thermal equilibrium, and the current from the Grove cells was passed alternately in opposite directions. The deflections are given in II. and III.

If the deflections in II. and III. are subtracted from the corresponding deflections in I., we get the amount of deflection due to the Thomson Effect. It will be observed that all the deflections in II. are less than those in I., and those in III. are greater, as they obviously should be. The only inaccuracy in this determination is due to the fact that we neglected the alteration in electrical resistance of the nickel due to the slight change in temperature.

I. Deflections taken every ½ minute.	II. H–C. Deflections every ½ minute.	III C–H. Deflections every ½ minute.	I. – II.	I. – III.
1·8	1·8	2·0	0·0	–0·2
2·6	2·4	2·8	0·2	–0·2
2·9	2·65	3·15	0·25	–0·25
3·1	2·75	3·25	0·35	–0·15

The numbers in these tables are obviously too small to draw any conclusions. They, however, confirm the preceding results as to the direction of the Thomson Effect; and tend rather to prove than disprove the reversibility of the effect. The experiment was repeated several times, but with no better result.

Experiments were also made to test the influence of mag-

netism on the Thomson Effect. Nothing but negative results, however, were obtained.

The strip of nickel was placed horizontally, with its flat surface perpendicular to the axis of a large electromagnet, the strip being between the two poles of the magnet. One surface of the nickel was pressed against one pole; on the other surface was placed one face of the thermopile, while the opposite face was in contact with the second pole of the magnet. Mica was used, as in the previous experiments, to protect the faces of the pile. The whole was wedged and pressed tightly together, and clamped by means of wire, the object being to prevent any motion of the nickel when the magnet was made. One end of the nickel was heated by a Bunsen burner; the other was at the temperature of the air. Six hours were required for the apparatus to reach a condition of thermal equilibrium. The electromagnet was connected with thirty-eight freshly set-up bichromate-of-potash cells, with plates of large size. A current from eight Grove cells was now passed along the nickel, with and without the circuit of the magnet being made. The deflections of the galvanometer were exactly the same in each case, showing that in a magnetic field (at least of the strength in the experiment) the Thomson Effect was unaltered.

It is unfortunate that the strength of the field could not be accurately obtained, as the batteries had been running about thirty minutes by the time the experiment was completed. The field, however, was very much stronger (as shown by rough tests) than in another experiment, where the minimum value was found to be 184 times the vertical intensity of the earth's magnetism.

The determination of the relative value of the Thomson Effect in nickel by the following method gives of course but approximate results. The value, however, is probably as accurate as those given by Le Roux for other metals.

A strip of copper, of about the same dimensions as the nickel used before, was arranged exactly as the nickel had been. The thermopile was insulated from the strip by the same piece of mica, and the same weights were placed on the upper surface. One end of the copper was heated in boiling water; and when the apparatus had reached a condition of equilibrium, the deflection of the galvanometer was 35 centim. A current from four amalgamated Grove cells was now passed alternately in opposite directions along the bar, the deflections of the spot being taken, in each case, after one minute. The results are given in the left-hand table :—

C–H. Deflections taken every minute.	H–C. Deflections taken every minute.	Differences between the corresponding deflections.	C–H. Deflections taken every minute.	H–C. Deflections taken every minute.	Differences between the corresponding deflections.
12·0 2	13·0 1	1·0	14·5 1	13·3 2	1·2
12·3 1	13·1 2	0·8	14·3 2	13·3 1	1·0
12·1 1	13·4 2	1·3	14·3 1	13·4 2	·9
12·0 2	12·8 1	0·8	14·8 2	13·3 1	1·5
Mean difference =0·97.			Mean difference =1·15.		

The strip of nickel was now substituted for the copper, every thing else remaining exactly the same. One end of the nickel was heated, and the thermopile was placed on such a spot that the galvanometer gave a deflection of 35 centim.

The same current was passed as above. The results are given in the right-hand table. Let $d=$ the mean difference in the first table, and $d'=$ that in the second, d and d' are then proportional to the elevation of temperature of the part of the bars under the pile on account of the Thomson Effect. Let $\sigma=$ coefficient of Thomson Effect; that is, σ is such a quantity that $\sigma d\theta$ represents the heat absorbed per unit current per unit time in passing from section at temperature θ to section at temperature $\theta+d\theta$. The heat evolved in unit section when the temperature is increased by $\frac{1}{2}d$. K is $\frac{1}{2}\mathrm{K}d\mathrm{SD}$, where K is a constant depending on the galvanometer, S is the specific heat, and D the density of the metal. If we consider the Thomson Effect to be constant under the pile, and θ and θ' to represent the temperature of the ends of the space covered by the pile, we have :—

$$\sigma(\theta-\theta')=\mathrm{K}\frac{d}{2}\mathrm{SD};$$

and the similar expression for nickel,

$$\sigma'(\theta-\theta')=\mathrm{K}\frac{d'}{2}\mathrm{S'D'}.$$

$$\therefore\ \frac{\sigma'}{\sigma}=\frac{d'}{d}\cdot\frac{\mathrm{S'}}{\mathrm{S}}\cdot\frac{\mathrm{D'}}{\mathrm{D}}. \quad \text{(I.)}$$

Equation (I.) then gives the relative value of the coefficient of the Thomson Effect at any temperature θ.

$$\mathrm{S}=\cdot095,\ \mathrm{S'}=\cdot108,\ \mathrm{D}=8\cdot9,\ \mathrm{D'}=8\cdot3,\ d=0\cdot97,\ d'=1\cdot15$$

$$\therefore\ \frac{\sigma'}{\sigma}=1\cdot25;\quad \therefore\ \sigma'=1\cdot25\sigma.$$

In Le Roux's table $\sigma=2$; $\therefore$ $\sigma'=2{\cdot}50$: σ and σ', however, are of opposite sign. Introducing nickel, Le Roux's table becomes:—

+		−	
Sb	64	Fe	31
Cd	31	Bi	31
Zn	11	Arg	25
Ag	6	Pt	18
Cu	2	Ni	2·25
		Al	0·1
		Sn	0·1

The Thomson Effect in carbon was next investigated. The carbon used was the graphite of the common carpenter's lead-pencil. The pencils which gave the best results were Faber's.

Attempts were first made to measure the direction of the Thomson Effect in the same way as in the case of nickel—that is, by placing a face of the thermopile on one surface of the carbon, the two ends of the carbon being maintained at constant temperatures, and passing the electric current alternately in opposite directions. This method was unsuccessful from the fact that one Grove cell heated the carbon to such a degree that in one minute the spot of light was thrown off the galvanometer-scale, thus rendering it impossible to measure, with any accuracy, the rate at which the deflection increased.

The method of Le Roux was then tried, of using two strips of carbon, each face of the pile being in contact with one strip. This method not only doubles the deflection due to the Thomson Effect, but also greatly diminishes the deflection due to the heat evolved on account of the electrical resistance of the carbon. If the two strips of carbon were exactly the same in all their physical properties, and the contacts with the faces of the thermopile were the same on each side, the latter deflections would evidently be entirely eliminated.

Two carpenter's pencils were split longitudinally, the lead being left in one half of the wood. They were then tightly bound, parallel, against each face of the thermopile, and insulated from it by thin pieces of mica. Especial care was taken to fasten the carbons firmly, so as to prevent any motion from the passage of the current. The pencils were placed perpendicularly, the lower ends in two vessels of mercury, surrounded by melting ice; the upper ends were at the temperature of the air. The upper ends were electrically connected; and the wires from a battery of three Grove cells were placed in the vessels of mercury. The thermopile was connected with a reflecting galvanometer of six ohms resistance.

When the system had reached a condition of thermal equilibrium, the current from the battery was passed, and the observations were made. The vessels of mercury and the corresponding pencils are denoted by "*a*" and "*b*." The current entered alternately in "*a*" and "*b*," the deflections of the galvanometer being taken, in each case, every half minute. The deflections showed that the pencil "*a*" was warmer than "*b*;" but the difference of temperatures was greater in one case than in the other.

The following table represents the results of two sets of experiments. The small numbers at top show which column of each pair was taken first.

First experiment.			Second experiment.		
Current enters at "*a*."	Current enters at "*b*."	Difference, proportional to 47 E.	Current enters at "*a*."	Current enters at "*b*."	Difference, proportional to 47 E.
[2] 21·0 34·5	[1] 20·8 32·4	0·2 2·1	[1] 20·8 34·7	[2] 20·2 32·8	0·6 1·9
[2] 21·2 34·5	[1] 21·0 33·0	0·2 1·5	[1] 19·5 31·0 37·0	[2] 18·2 29·3 34·2	1·3 1·7 2·8
[2] 21·4 34·3	[1] 20·6 32·8	0·8 1·5	[1] 19·8 31·8 37·3	[2] 18·0 29·2 34·0	1·8 2·6 3·3
[2] 21·7 36·0 42·4	[1] 21·6 34·3 40·0	0·1 1·7 2·4	[1] 20·0 32·6 38·7	[2] 18·8 30·4 35·7	1·2 2·2 3·0
[2] 23·0 38·2 45·5	[1] 21·7 35·0 41·2	1·3 3·2 4·3	[1] 21·0 33·8 39·8	[2] 19·8 31·3 36·3	1·2 2·5 3·5
[2] 23·5 39·0 45·8	[1] 23·0 37·0 43·8	0·5 2·0 2·0	[1] 20·0 32·2 37·4	[2] 19·0 29·8 34·3	1·0 2·4 3·1

From this table it appears that the Thomson Effect in ordinary graphite is negative ; that is, heat is apparently evolved when the current passes from cold to hot, or the negative current carries heat with it. The difference in the last columns are obviously proportional to four times the Thomson Effect,

assuming that the effect is reversible. It also appears from the table that the effect increases as the temperature increases, which is in accordance with Tait's assumption.

These experiments were repeated with the graphite from other kinds of pencils; but in no case was the effect nearly as marked as in Faber's. Even in the case of Faber's pencils many trials were made before satisfactory results were obtained.

Equations representing the thermal condition of a bar when acting as a conductor of heat and electricity may be deduced as follows:—One end of the bar is supposed to be maintained at a constant temperature, the other at that of the air; and the electric current is supposed to be constant. For simplicity, we will assume that the specific electrical resistance of the bar is constant throughout, *i. e.* is independent of slight differences of temperature.

The quantity of heat, H, evolved by the current in time δt, in the section of the bar $S\delta x$ (S being the area of a section), is represented by

$$H = I^2 RS\delta x \,.\, \delta t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{I.})$$

$x =$ distance of the section from heated end. If we assume that the thermal conductivity is unaltered by the slight rise in temperature due to the current, it can easily be seen that the flow of heat due to conduction is unaltered by the current. Hence we can consider that the heat evolved by the current is partly used in raising the temperature of the section $S\delta x$, and that all the rest escapes from the surface by radiation.

The Thomson Effect is at present purposely neglected.

The bar is supposed to have reached a permanent condition as regards conduction before the current was passed. Let θ be the temperature of the section of the bar we are considering before the current passes; let $h =$ the exterior conductivity or velocity of cooling; let $p =$ the rise of temperature above θ when the current passes. Assuming Newton's law of cooling, the heat radiated on account of the rise of temperature p is proportional to ph; and the quantity radiated from the section in time δt from the same cause is

$$H_1 = phl\delta x \,.\, \delta t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{II.})$$

$l =$ the periphery of the bar.

In time δt the increase of temperature p becomes $p + \delta p$; and the heat developed in the section by this increment is

$$H_2 = CSD\delta x \,.\, \delta p. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{III.})$$

As we saw that the heat of the current was expended only

in the ways represented by (II.) and (III.), we have

$$H = H_1 + H_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (IV.)$$

If we now consider the influence of the Thomson Effect, we simply add that a certain quantity of heat is absorbed or evolved by the current in the section $S\delta x$, distinct from that represented by I^2R.

If σ = the coefficient of the Thomson Effect, the heat absorbed or evolved due to this effect is, in time δt,

$$H_3 = I\sigma\delta\theta \, . \, \delta t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (V.)$$

The effect being proportional to the current, and σ being defined as such a quantity that $\sigma\delta\theta$ represents the heat absorbed or evolved in passing from a point at temperature θ to $\theta+\delta\theta$, per unit current per unit time, introducing this effect in (IV.),

$$H = H_1 + H_2 + H_3, \quad . \quad . \quad . \quad . \quad . \quad . \quad (VI.)$$

as the total value of the excess of heat (due to the current) in the section can be considered as made up of these quantities. Substituting the values in (VI.) from (I.), (II.), (III.), (V.), and transposing,

$$plh\delta x \, . \, \delta t = I^2RS\delta x \, . \, \delta t - CSD\delta x \, . \, \delta p - I\sigma\delta\theta \, . \, \delta t \, ;$$

$$\therefore \; phl = I^2RS - CSD\frac{\delta p}{\delta t} - I\sigma\frac{\delta\theta}{\delta x},$$

or, at the limit,

$$\frac{dp}{dt} = \frac{1}{CSD}\left[I^2RS - phl - I\sigma\frac{d\theta}{dx}\right]. \quad . \quad (VII.)$$

This equation gives the rate at which the temperature rises when the current passes, and will approximately apply to the preceding experiments.

When the temperature of the bar becomes permanent,

$$\frac{dp}{dt} = 0,$$

and (VII.) becomes

$$I^2RS - phl = I\sigma\frac{d\theta}{dx} = 0;$$

$$\therefore \; p = \left[I^2RS - I\sigma\frac{d\theta}{dx}\right]\frac{1}{hl}, \quad . \quad . \quad . \quad (VIII.)$$

giving the excess of temperature due to the current in the permanent condition of the bar.

The values in (VIII.) are all easily determined except σ and h. The differential coefficient $\frac{d\theta}{dx}$ (the rate of change of

temperature due to conduction along the bar) can readily be found by experiment; or deduced by analysis, as in the case of an infinite square bar, where

$$\theta = a\mathfrak{S}^{-kx} \text{ and } \frac{d\theta}{dx} = -ak\mathfrak{S}^{-kx}.$$

As p may easily be determined by experiment, the equation can be used to determine σ, as

$$\sigma = \frac{I^2RS - phl}{I\frac{d\theta}{dx}}. \quad . \quad . \quad . \quad . \quad . \quad \text{(IX.)}$$

If Tait's assumption that $\sigma = MT$ (where M is some constant and T the absolute temperature) is true, we might obtain two values of σ for two points of the bar, the temperature of which was known, eliminate h from the two equations, and thus obtain a value for M. If we performed the same operation for two other points, we should get another value for M, and could verify Tait's assumption if this value was equal to the preceding.

The sources of error in the preceding investigation are due to assuming Newton's law of cooling, to neglecting the change of electrical resistance due to a change of temperature, and to partly neglecting the change of thermal conductivity due to the same cause.

L. *On the Reflection of Electrical Rays.* *By* Dr. E. Goldstein *.

[Plate VII. figs. 1–8.]

IT has been usually assumed † that the (rectilinear) electrical rays radiating from the kathode of the discharge of an induction-coil terminate where they impinge upon a solid wall, and that beyond the point in which they cut the wall they cannot propagate themselves in any direction ‡. The experi-

* *Monatsber. der Königl. Akademie der Wissenschaften zu Berlin.* Translated from a separate impression communicated by the Author.

† Hittorf, Pogg. *Ann.* cxxxvi.

‡ Herr J. Puluj (*Wien. Ber.* 1880, [2] p. 886) is the only physicist who has assumed a limited power of reflection of the kathode-rays, under the assumption that the kathode-light consists of scattered particles of the electrode, since "it is not intelligible why these should in general suffer no reflection at the wall." The conditions of an experiment made by Herr Puluj to examine whether reflection takes place were not, in my opinion, such that any possible reflection would have been recognizable. That which Herr Puluj considers phosphorescence produced by reflected rays is partly phosphorescence produced by the positive light of the so-

ments which have led me to reject this assumption were suggested by an observation made by Prof. E. Wiedemann*.

Prof. Wiedemann, in using a tube of the form of fig. 1 (Plate VII.), where the disk k at right angles to the axis of the vessel C forms the kathode, not only observed green phosphorescence such as produced by the kathode-rays on the sides of the tube up to the point x which could be reached by straight lines from k, but saw also a feeble illumination of the tube r beyond the bend, and a brighter phosphorescent surface F on the wall C opposite the mouth of the tube r. The motions of the small surface F under the influence of the magnet showed that it was produced directly by electrical rays, and not simply by optical rays possibly reflected at the glass.

Prof. Wiedemann is disposed to explain the appearance as one of the phenomena of deflection discovered by me †, assuming that the glass wall at x becomes charged and acts as a weak kathode, causing the deviation of the pencil of rays passing by it out of the direction at right angles to the plane of k, into the direction xF.

This explanation seemed improbable to me for two reasons:—

(1) Because the surface F is always much more feebly illuminated than would have been the case if the phosphorescence had been exerted by the direct kathode-rays penetrating to C. In order to make the comparison, the kathode-rays may be so curved by the influence of a weak magnet as to pass the bend x. The comparison may be more certainly made without the use of a magnet in a vessel of the form shown in fig. 2, where two paths are offered to the kathode-rays—on the one side the path as in fig. 1 through the bent tube $r\,r_1$, and on the other side the path through the equally long straight tube r_2 at right angles to k.

(2) Because the surface F totally disappears if the tube $r\,r_1$ has a second bend in it, in whatever direction this second bend is made. This would not happen with the phenomena of deflection, which I have examined, where a ray may be bent any number of times.

If now the rays which produce the surface F are not direct rays from the kathode k, then we may suppose either (a) that the portions of the tube about x, upon which the rays from the

called reflex currents in the system of tubes, partly phosphorescence produced by direct kathode-rays, which Herr Puluj unintentionally produced by touching the glass with the finger in order to concentrate the light.

* E. Wiedemann, Wied. *Ann.* xi. p. 236; Phil. Mag. [5] x.

† Goldstein, *Monatsber. d. Königl. Akad. der Wiss.* 1876, p. 285; Phil. Mag. [5] iv. Also 'A new Form of Electrical Repulsion' (Berlin, 1880).

kathode impinge directly, become charged with negative electricity, which reaches such a tension that they themselves form a second kathode and radiate electric rays, which then produce F; or (*b*) that the rays which produce F are rays from the kathode *k*, which suffer reflection when they fall upon the solid wall. In this reflection, either the power of producing phosphorescence of the rays becomes weakened or their density, thus explaining the small intensity of light emitted by F.

The hypothesis (*a*) may be excluded, as shown further on; for the phenomenon in question is not altered if the surface upon which the kathode-rays impinge directly be metallic, and if this metal surface be made the anode of the discharge.

If in accordance with this assumption we suppose that reflection takes place, then again reflection according to the optical law is at once to be excluded, since the position and form of the surface F remain unchanged even when the angle of the bend at x varies from 25° to 80°. Consequently the law of the equality of the angles of incidence and reflection, or the rule that when the reflecting surface is rotated while the incident ray preserves the same direction, the reflected ray rotates through twice the angle, is not obeyed.

We may, however, easily make numerous experiments which agree in showing the presence of diffused reflection, in consequence of which each point of the wall on which the rays impinge directly diffuses rays in all directions.

If diffuse reflection is proved, we have at once the explanation of the small luminosity of F in comparison with portions of the tube reached by the direct rays in the diminution in density of the incident pencil of rays.

In the next place, if we employ a vessel such as fig. 3, we obtain phenomena corresponding to the surface F in the cylinders C_1 and C_2 at the same time. The rays which travel as far as x nearly parallel to each other, therefore, after reflection, follow at least two directions at right angles to each other.

The following experiment forms an *experimentum crucis*:— A chamber B was introduced between the portion of the tube containing the kathode and the bend at x, which contained a paper diaphragm which could be turned round the axis a, and which had a slit cut in it about 1 millim. broad and parallel to a. If the plane of the diaphragm falls along the axis $r\,r$, its edge intercepts no perceptible portion of the pencil of rays which reaches the tube r from the kathode, and which is about 7 millim. across. But if, on the other hand, D is at right angles to the axis of r, then only the portion of this pencil which passes through the narrow slit can reach the bend x

If the reflection were similar to that of a mirror, the form and magnitude of the surface F would change perceptibly. This surface, in all the experiments so far described, and in this one also, resembles an ellipse of small excentricity, where D has the position first mentioned. Its smaller axis, which falls in the plane of $r\, r_1$, is about twice as large as the diameter of r_1 in the vessel C, which is about 3 centim. in diameter. If now we mark the position, form, and magnitude of F on the outside of the tube C when the diaphragm presents its edge only to the kathode-rays, and then place the diaphragm at right angles to the rays, we find that *the position, form, and magnitude of* F *remain unchanged*; only its *luminosity* is now considerably diminished.

If, instead of the diaphragm with a slit, a plate without openings is introduced into the chamber B capable of free motion, so as to cut off at pleasure either the upper or lower half (and also the right-hand or the left-hand half of the kathode-pencil) by covering the corresponding portion of the mouth of r, then also the position, form, and magnitude of the surface F remain unchanged; the luminosity only of the whole surface decreases, but most in the half which is opposite to the half that has been intercepted. Thus, for example, the surface is darkest in the upper half when it is the lower half of the kathode-pencil which is intercepted.

We easily see how these observations, inconsistent with optical reflection, entirely agree with the assumption of a diffuse reflection of the kathode-rays.

Tubes of the form of fig. 5 are better adapted for the further study of this diffuse reflection than the vessels employed by Wiedemann. The rays emitted by the kathode k which pass through the connecting tube r into the wider cylinder Z fall then upon the plate P, which is fastened to a wire d insulated with glass inside Z. The cylinder is closed air-tight by the caoutchouc stopper k, by removing which the plate P can be exchanged for another; or other changes in the apparatus can be made.

If the plate P consists of phosphorescent glass, then the rays which fall upon it directly produce at the plate s simply an oval very bright green phosphorescent surface. We see, however, distinctly how the diffuse reflection from this surface causes the whole wall of the tube Z lying above the plane of P up to the stopper k to phosphoresce with subdued green light, which is weaker the further the portion of the wall is removed from s.

If the plate P be covered with chalk, its surface at s shines with orange-red light, but the wall of Z presents a green

luminosity as before—a proof that this luminosity does not depend upon optical reflection. So also the phosphorescence produced by diffuse reflection remains unaffected if P be constructed of some material which does not phosphoresce at all. It is further a matter of indifference whether P is metallic or consists of an insulator. In the former case P may even be made the anode of the discharge, without the reflection of the rays appearing in any way weakened.

The kathode-rays are therefore not absorbed by the anode, even when they play directly upon the surface of the anode. Further we see, as already mentioned above, that the phenomenon in question cannot be explained by supposing that the surface struck by the direct kathode-rays is itself converted into a kathode.

If we bring small objects between the plate P and the phosphorescent surface of Z, such, for example, as the wire D (fig. 6), whose distance from P can be varied by rotation round the axis D, we can very well recognize the character of the diffuse reflection which the place *s* causes in the rays which fall directly upon it. For the shadow of the wire D only appears narrow and sharp when the wire is brought close to the wall of the vessel; if D is moved from the wall towards P, its shadow soon becomes broad and indistinct. If we cut off a further portion of the kathode-rays, by means of a small movable plate of mica introduced into Z at the mouth of *r*, the space *s*, directly impinged upon by the rays, of course becomes smaller. The further this decrease proceeds the narrower and sharper does the shadow of D become, exactly as we should expect on the theory of diffuse-reflection.

I will here cite only one other consequence of this theory which has been experimentally verified. I may take it as known that a pencil of rays emitted by a plane kathode after it has passed, as in fig. 7, through the aperture (supposed circular) of a diaphragm occupying the whole area of the tube, gives on the flat wall W a well-defined circular luminous figure on a dark ground.

Upon our assumption of the diffusion of the kathode-rays, this ought not to happen any more if the rays are made to pass through a cylindrical tube open at both ends (fig. 8), in place of the thin diaphragm. For since the different rays of a plane kathode are not altogether parallel to each other, but also diverge somewhat around the central portion of the kathode, a part of them must play upon the wall of the tube *r*, and be then diffusely reflected. The portion of the diffuse rays which reach C must then form an extended luminous space round the bright surface resulting from the direct rays. We find

this confirmed by experiment; and if we bring into C a wire δ throwing a shadow, we find that its shadow is sharp and narrow in the region illuminated by the direct rays, but broader and ill defined in the surrounding region. This takes place also when δ is made the anode. We see from this that the difference in breadth of shadow does not depend upon a stronger deflection which δ, apparently neutral but really acting as a weak kathode, causes in the reflected rays*. These last are indeed themselves capable of deflection, as we see if, instead of making δ an anode, we connect it with the earth, or give to it a small portion of the current from the kathode.

The motions of the shadow of δ under the influence of a magnet, and with other arrangements the motions of the surface F under similar influence, show that the reflected rays are deflected, as far as one can observe, in the same way as the direct kathode-rays would be if their course were the same as that of the reflected rays.

If r be placed equatorially above a horseshoe magnet of suitable strength, the direct kathode-rays before reaching C are compressed against the upper or under wall of r, and the phosphorescent surface (ϕ) produced by the direct rays disappears; but the feebly illuminated region remains, occupying now the position of the surface ϕ: this corresponds exactly to Wiedemann's surface F; its production here is due to the diffuse rays which issue from the portions of the straight tube r struck by the magnetized rays. The further the terminal point of the direct rays is forced towards C by the action of the magnet, the less luminous does the surface F become, since then a continually smaller portion of r is able to reflect rays.

On the whole, the foregoing series of experiments leads to the following result, which I propose to describe more fully in a further communication:—A pencil of kathode-rays does not end (at any rate under the conditions suitable for producing phosphorescence) where it strikes upon a solid wall, but electric rays radiate in all directions through the space occupied by the gas from each point of the wall struck by the direct rays. These rays may be called reflected rays. Any solid wall, no matter what its properties are, may serve as reflecting surface. It is a matter of indifference whether it is capable of becoming phosphorescent or not, whether it consists of a conductor or of an insulator. The reflection is diffuse, equally whether the reflecting surface is dead or as smoothly polished as possible. An anode apparently reflects the kathode-rays, the same as a neutral conducting surface or an insulator. The reflected rays, like the direct kathode-rays, have the property

* 'A new Form of Electrical Repulsion,' p. 124.

of exciting phosphorescence at their ends. They are capable of being deflected ; and their ends are bent aside by a magnet in the same direction as the ends of the kathode-rays would be which radiated from the reflecting surface to the points reached by the reflected rays.

LI. *On the Influence of the Shape of the Kathode on the Distribution of the Phosphorescent Light in Geissler's Tubes. By* Dr. E. GOLDSTEIN*.

[Plate VII. figs. 9–35.]

CYLINDRICAL wires cut off at right angles have been almost exclusively employed as kathodes in systematic investigations on the discharge of the induction-coil in rarefied gases, or, in particular cases, spherical electrodes or plane circular disks. Kathode-surfaces, which can be divided into two halves of similar shape by an infinite number of cuts, do not give rise to a class of phenomena which I have observed with kathodes of regular surface, in which nevertheless there is no axis of symmetry corresponding to an infinite number of equivalent sections.

We are concerned with extremely regular figures, in which the phosphorescent light of the walls illuminated by the raysfrom those kathodes arranges itself, which, however, are for the most part altogether unlike the shape of the kathode itself. Reserving a detailed description, I may here give the general characters of the most important types of these figures †.

Kathodes of concave spherical form were first examined constructed of thin soft iron, which was first of all stamped and then ground into the desired form.

The kathodes were soldered at the middle points of their convex sides to wires which conveyed the current, and which were insulated by being covered with glass thermometer-tubing between their junction to the kathode and the point at which they entered the vessel.

The discharge-tubes were glass bulbs of 4 to 5 centim. radius; the axis of the spherical concave mirror which formed the kathode was placed in a diameter of the bulb. The distance of the kathode from the wall, measured along this diameter from the centre of the mirror, could be varied ; in the

* *Monatsber. der Königl. Akademie der Wissenschaften zu Berlin*, July 1881 Translated from a separate impression communicated by the Author.

† A preliminary notice appeared in the *Wien. Akad. Anzeiger* of the 13th Jan. 1882. The phenomenon of figures in phosphorescent light dissimilar from the kathode was described by me for the case of a kathode of cylindrical curvature so long ago as 1876 (*Wien. Sitzungsber.* lxxiv. [2] p. 465).

experiments next to be mentioned it was made equal to twice the radius of curvature of the spherical kathode.

If we now assume, as for example Crookes does in his well-known memoirs, that from each point of a concave kathode only one rectilinear ray radiates, and that along the normal to the surface, it would follow that the phosphorescent image of a concave kathode on a concave spherical wall, at a distance of twice the radius of curvature of the kathode, would be identical in form and dimensions with the kathode itself, if the radius of the vessel were equal to that of the kathode; it would be coincident in form and nearly in dimensions with the kathode itself if, as in my experiments, the radius of the vessel were greater than that of the kathode, without the kathode having any considerable aperture. There will be no essential change in the character of the phenomena to be expected, if we also take into account the feebler phosphorescence caused by the rays* emitted by the elements of the kathode on its edge in variously oblique directions up to the tangential direction. But experiments show very different phenomena.

1. Fig. 9 *a* represents a square of the actual size, ground into a spherical surface of 40 millim. diameter; and fig. 9 *b* represents the phosphorescent image, also of the actual size, formed by this kathode in a highly exhausted glass vessel of 8 centim. diameter; we remark a star of light with four rays, the axes of the rays being at right angles to the sides of the square kathode. In the figure representing the luminous star, the edge of the kathode is marked by black dots in order to indicate the relative positions of kathode and image. At extreme exhaustions there appear, less distinctly marked, four much shorter rays coming from the centre of the image and corresponding to the directions of the diagonals of the kathode†.

An equilateral triangle having the same curvature (fig. 10) produced a star with three rays, whose axes were at right angles to the sides of the triangle. So also polygons of 5, 6, 7, and 8 sides gave stars, with a corresponding number of rays, whose axes appeared to bisect the sides of the polygon at right angles.

The position of these figures with reference to the kathode

* *Eine neue Form electrischer Abstossung*, i. p. 11.

† In the accompanying figures of portions of the surface of a sphere, the arcs of great circles between the centre of the light-figure and the separate points forming the bounding surface of the figures are approximately represented by the chords of these arcs, or in the smaller figures by the corresponding aliquot parts of these chords. This corresponds with the method of measurement employed, in which distances on the spherical surface were determined by the direct distance between the points of a pair of compasses applied to the surface.

is worthy of remark, as throwing light upon their mode of production. If we cover up the upper portion of a polygonal kathode by a screen placed between the kathode and the centre of curvature, then the *upper* arms of the star are wanting in the image. The arms are therefore not produced, as we might have expected, by the radiation from the portions of the kathode diametrically opposite.

A four-armed cross, fig. 11 *a* (actual size), forming a portion of a sphere of 40 millim. diameter, gives fig. 11 *b* on the wall of a vessel 8 centim. in diameter, in which again the position of the kathode is marked by dots. The metallic arms of the kathode thus correspond to the dark arms of the cross in the phosphorescent figure, and the light is concentrated in fields corresponding to the intermediate spaces in the kathode.

If, again, the kathode has three arms, with angles of 120° between the arms, we obtain a figure with three dark arms. These again fall upon the metallic arms, and the bright fields upon the intermediate spaces between the metal arms *.

The dark arms of the image, however, are much narrower with the four-rayed figure than with the three-rayed one, if the arms of the kathode are of equal width and equal length in the two cases.

We obtain analogous figures when the kathode has five or six (uniformly distributed) arms, except that as the number of arms increases the dark fields which correspond to the metallic arms become narrower, not only absolutely but also relatively —that is, in proportion to the width of the bright intermediate fields.

These two typical forms—the polygon, and the star consisting of rectangles—may suffice as a preliminary indication of the forms of the images which appear when the exhaustion is sufficient.

2. The images formed by kathodes of this sort alter very much when the density of the gas is altered.

The image-forms described above are obtained with densities of gas about $\frac{1}{100}$ millim. mercury. We can, however, trace the phosphorescence at pressures only slightly less than 1 millim., or even over this pressure, by including in the discharge at the same time sparks in free air. We obtain then, for example, for the four-armed cross-shaped kathode, fig. 11 *a* (radius of curvature 20 millim.), the images 12 *a* to 12 *e* in

* I have made repeated attempts to obtain a result described in Carl's *Repert.* 1880, p. 244, where a sort of three-rayed star gave simply an erect image of the kathode, but never with success.

succession, to which succeeds the image represented in fig. 11 *b* when the exhaustion becomes sufficient.

We obtain then at first an image of the kathode itself—a cross having nearly the dimensions of the cylindrical projection of the kathode upon the wall. The arms of the cross in the image grow smaller and become the central lines of a well-defined square, that appears as a luminous background round the cross. As the density decreases, the square becomes smaller and its luminosity increases. Its sides lengthen beyond the angular points, and form points resting on the sides of the square. The central cross disappears; only the intersection of its arms remains as the bright centre point of the whole figure.

The square now becomes smaller; and the points superposed on the sides become narrower in the same proportion. This takes place in consequence of the concave sides of the contour-lines approaching each other, and passing over each other, 12 *d* and 12 *e*. As this displacement continues in the same direction while the density of gas decreases, the pair of arcs which previously intersected finally pass apart, and leave a dark space between their convex sides: thus finally, at great exhaustion, the dark cross already figured in fig. 11 *b* results.

The series of changes thus described is typical also for the successive images given by crosses or stars with other numbers of arms.

We obtain in each case at first an image closely resembling the kathode, and of nearly the same dimensions. Observation with three- and four-rayed stars (fig. 13 and fig. 14), shows—what was not evident with the four-rayed star, nor generally with regular figures with an even number of arms—that these figures are reversed images of the kathode, formed by the rays from the kathode crossing each other. Next, the background round these figures becomes brighter, bounded by as many sides as the kathode-star has rays. The rays of the star-figure form the smaller radii of the polygon so formed. As the exhaustion of the gas continues, this polygonal figure decreases, its surface becoming brighter; and the inserted star disappears, its bright centre point remaining visible the longest. On each side of the polygon appears a point similar to those in fig. 12 *d*. The concave sides of the bounding arcs approach each other more the further the exhaustion proceeds; and at last, as the result of displacements exactly similar to those of figs. 12 *f* and 12 *g*, there appear dark rays between bright fields, which again correspond to the metallic arms of the kathode.

The type of successive changes in decreasing density of gas with polygonal kathodes may be illustrated by the series of images given by a concave spherical square (fig. 15). Figs. *c* and *d*, for the sake of greater distinctness, are represented on a scale somewhat larger. If the kathode-polygon possesses an odd number of sides, then the luminous polygonal images corresponding to figures 15 *b–d* are reversed with reference to the polygonal kathodes, corresponding with the observation made with star-shaped kathodes with an odd number of rays.

It is these luminous figures so far described and figured which first strike the observer, in the phosphorescent images of the corresponding kathodes; upon closer observation we see that the other surfaces of the glass vessel are also not devoid of luminosity, but show phosphorescent surfaces of feeble luminosity at different points. The boundaries of these surfaces, in the case of star-shaped kathodes, are prolongations of the luminous curves which bound the chief figures; with polygonal kathodes they are extensions of the star-rays observed in the luminous figure. Partly because, from what has been said, a sufficient preliminary account of the way in which these less-luminous portions complete the figures already figured and described is now possible to the reader, and partly to economize space, I abstain from further description of these outlying portions until the separate observations have been completely described. The image obtained in a spherical vessel of about 9 centim. diameter, employing as kathode the cross figured in fig. 11 *a*, may serve as a good example: it is represented in figs. 16 *a* and 16 *c*, in the pointed phase and in the dark-cross phase.

3. Besides the forms obtained by simple variation of the number of sides and arms of polygons and stars composed of rectangles, I have further examined the images given by numerous other forms of kathode—some simpler, some more complex—in order to separate as far as possible that which is general from that which is special. Thus, for example, of simpler forms were examined:—rectangle, rhombus, rhomboid, isosceles right-angled triangle, &c. of compound forms; crosses composed of obliquely compounded rectangles, and crosses formed of isosceles triangles (the latter either with the vertices or with the bases outwards); further, figures such as are obtained by cutting out of squares segments of circles or smaller squares at the four sides.

So far as the effect of the form of the kathode, and of the variation in density of gas, manifests itself with these images, I must defer a description of the phenomena observed until I give a more complete explanation of the whole. The

following general rules (4–7), however, hold good for all the kathode-forms mentioned.

4. At constant density of gas, the forms of the phosphorescent images (not only their absolute dimensions) alter when the distance of the kathode from the wall of the vessel is made to change; as the distance of the wall decreases, the same figures appear, in the same order, as when the distance of the wall remains constant and the density of gas decreases.

Instead of altering the distance of the wall by displacing the kathode, we may, as in the vessel represented in fig. 17, displace the wall which receives the rays with reference to the kathode.

If we experiment with varying distance of wall, and also with varying density of gas at the same time, then, in order to pass from one given figure to another of the same series, the wall must be displaced through a greater distance the smaller the density of gas is.

This shows that all the figures which a kathode can call forth upon a fixed wall as the density of gas decreases, do at any fixed density of gas already exist in space one behind the other at the same time, and that the different figures are produced by the rays intersecting each other in various ways at different points of space. As the density of gas decreases, the images move further apart and further away from the kathode, no doubt because the rays which first converge become less convergent, and then, when after intersection they diverge, their divergence is decreased.

The influence of the distance of wall thus described would lead us to expect that the series of images given in figs. 12 and 15 as examples obtained with a kathode at a distance equal to twice the radius of curvature, would not include all the forms which the particular kathode is capable of producing, but that a diminution of the distance of the wall at the highest obtainable exhaustion would in general give other figures besides these. This conclusion is found to be confirmed by experience, although the forms thus obtained are not for the most part so striking as those previously described. It may, however, be mentioned at this stage as worthy of consideration further on, that upon diminishing the distance of the wall the dark arms of the cross in fig. 11 *e* increase considerably in width.

5. If we take similar plane figures, and then bend them into portions of spheres of different radii, ρ_1, ρ_2, ρ_3, and place them as kathodes in similar vessels with equal distance of wall, then at equal density their images represent different phases of the series of figures obtained from a single such

kathode with varying distance of wall; and the figure produced by a particular kathode k corresponds to a greater distance of wall from the kathode used to compare with, the greater the curvature of the kathode k is.

This result might indeed have been regarded as *à priori* probable. We might indeed expect to obtain simultaneously like figures from different kathodes (similar in their original plane condition) by making the distance of the wall equal to $n\rho$, $n\rho_1$, $n\rho_2$, &c. for different radii of curvature ρ, ρ_1, ρ_2, &c. —that is, the distance of the wall in each case the same multiple or submultiple of the radius of curvature—as, for example, by placing each kathode at a distance from the wall equal to twice its radius of curvature.

But experiment shows also that in this case the phases are different; and the increased curvature acts in the same way as, *cæteris paribus*, the increase of the distance of the wall or an increase of the density of gas. This influence goes so far that, with kathodes which are much curved, it has not been found possible by exhausting the gas to produce those forms of the series of figures which, with electrodes of less curvature, correspond to the lowest degrees of the scale of density. Thus, for example, with the four-armed cross fig. 11 *a*, of a radius of curvature of $12\frac{1}{2}$ millim. instead of 20 millim., and with a distance of wall 2ρ, we find it impossible by exhausting to reach the phase of the dark cross fig. 11 *b*. The figure obtained immediately before the cessation of the current at the greatest exhaustion is the figure with curved points, fig. 12 *c*.

6. If we leave the general form of the kathode and its curvature unaltered, but increase the aperture of the kathode, this increase acts also as an increase of the distance of the wall would do.

If, for example, we replace a square of 12 millim. in the side which has been bent to form a spherical surface of 40 millim. diameter by a square of similar curvature, but with sides 30 millim. long, then at the extreme exhaustion we do not advance further than fig. 15 *d*, whilst the small square gives us figures up to 15 *g*.

We obtain similar results with the more complicated forms of kathodes—for example, the four-armed cross made up of rectangles, fig. 11 *a*. If the length of the cross be increased from 20 to 25 millim. without altering the width of the arms, then at the greatest exhaustion at which the current will pass, the dark cross is just visible; but it cannot be obtained with arms of any considerable breadth.

If the cross is increased to 40 millim. the dark cross is no

longer to be obtained, and fig. 12*c* is the last obtained at extreme exhaustion.

The results are qualitatively identical if we increase the breadth of the arms in the same proportion*.

7. The experiments mentioned in No. 5 show that the phase alters when plane kathodes of similar shape are bent into spherical surfaces of different curvature. Since the same figure, formed to different spheres, will form a mirror of greater aperture the smaller the radius of the sphere is, the result described in No. 5 as to the influence of increased curvature might possibly seem to be only another statement of the influence mentioned in No. 6 of increased aperture. In that case we should expect that kathodes of like form and different curvature but like aperture, at distances forming the same multiples of their radii of curvature, would give like figures at like densities of gas.

To test this I constructed a series of kathodes (*e. g.* three four-armed crosses, I., II., III.), whose dimensions were as follows:—

	Radius of curvature (ρ). millim.	Length. millim.	Breadth of arms. millim.
I.	$12\frac{1}{2}$	$12\frac{1}{2}$	$2\frac{1}{2}$
II.	20	20	4
III.	$26\frac{3}{4}$	$26\frac{3}{4}$	$5\frac{1}{3}$

The kathodes thus all covered equal aliquot parts of the spheres from which they were formed. They were placed at a distance 2ρ from the spherical wall of the similar containing vessels—that is, 25 millim., 40 millim., and $53\frac{1}{2}$ millim. respectively.

Here also the result was obtained that there is no identity of phase for equal density, but the figures given by kathodes II. and III. corresponded to the figures which kathode I. would have given if the distance of the wall had been increased. This occurred indifferently, whether the two electrodes of each of the three discharge-vessels were separately connected with the poles of the induction-coil, or whether, in order to secure equal intensity of discharge, the current was sent at the same time through the three vessels, connected together in line.

* The changes in curvature and aperture of kathode described in the two preceding paragraphs cannot, in one respect, be always compensated by changes in density of gas or in distance of wall: the last term of the series of figures (corresponding to fig. 15*g*) for a polygonal kathode shows with kathodes of greater curvature or larger aperture richer differentiation and finer detail. The rougher figure cannot be reproduced with these kathodes.

8. The phosphorescent figures which appear when spherical electrodes are employed, as might have been expected, become replaced by others when the originally plane kathodes are formed, not into spherical surfaces, but into cylindrical or conical surfaces. I will content myself in this cursory report with mentioning that, with cylindrical kathodes, the phosphorescent images are different according to the position which the axis of the cylinder has with reference to the axis of symmetry of the kathodes. Thus, for example, with a square kathode formed to a cylindrical surface we obtain different figures according as the axis of the cylinder is parallel to a side of the square or to one of its diagonals. In the same way there are marked differences in the images of cross-figures according as the axis of the cylinder is parallel to one of the arms of the cross, or bisects the angle between the arms.

9. With reference to the explanation of the phenomena so far described, it is only wise to maintain a certain reserve towards a class of phenomena as yet very imperfectly known. A doubt as to the success of the attempt to bring a large portion of these phenomena into any simple relationship with known causes, could only be supported by the observations described in No. 10, on the images given by plane kathodes. All that I hope to do is to give some indications, derived from experiment, on the direction in which the explanation of many of the phenomena observed with star-shaped kathodes is to be sought. As to the explanation of most of the phenomena given by the above-mentioned polygonal kathodes, I do not here venture upon any hypothesis. The star-shaped kathodes are of course also polygons with reentrant angles; and their separation from the polygonal surfaces appears unnatural at first sight. Nevertheless the phenomena which we have to explain with star-shaped kathodes are for the most part just those which are closely connected with the presence of reentrant angles, and which may be approximately explained by taking into account only the edges of the kathode. The phenomena due to the surfaces themselves, in those forms of kathode where the surface of the kathode is relatively unimportant as compared with the extent of its bounding edge, are insignificant in comparison with the phenomena due to the curves of its circumference ; and the former remain unexplained here just as with the polygons with convex angles, where the phenomena of the surface constitute the chief part of the phenomena observed. Only the surface-phenomena produced at small exhaustions can be approximately accounted for with both kinds of polygons That the images of a star-shaped kathode which appear at relatively small exhaustions result

from the intersection of the rays emitted by one half of the cross with those from the other half, was shown to be probable by the experiments already described with stars of an uneven number of arms. This may, however, be better shown by experiments with screens arranged to throw shadows, which also prove the same thing for kathodes with an even number of arms.

If we arrange (fig. 18 *a*) a screen of paper or of mica in front of the kathode, and at a distance from its centre less than the radius of curvature, so that, for example, the lower half of the kathode-cross is covered, then in the phosphorescent image it is the upper half of the cross which is wanting (fig. 18 *b*).

Since at a certain density these images formed on the wall at the distance 2ρ from the kathode possess nearly the same dimensions as the kathode itself, it follows that the rays which produce the images, at least for the most part, are under these circumstances at right angles to the surface from which they are emitted, assuming, of course, that their course till they strike upon the walls of the tube is rectilinear.

(Exactly similar experiments show that the images of ordinary polygonal kathodes observed at small pressures are formed by the intersection of the rays which issue from points situated symmetrically with reference to the centre of the kathode.)

As with the figure of the kathode-cross, so also in the contour of the square (fig. 12 *b*) which appears surrounding the figure of the cross as the density decreases, each side is formed by rays which come from the opposite side of the kathode.

We may obtain a more satisfactory explanation of the figures of kathode-crosses which appear at small densities; we only need to place a screen P (fig. 19 *a*) between two of the arms of a cross, so that it projects beyond the kathode on the concave side. The screen may be rectangular, and the edge which faces towards the image may be called its front edge. I anticipated, and the anticipation was verified by experiment, that the figures which correspond to the higher degrees of exhaustion are formed by the rays emitted by one arm of the kathode suffering a repulsion by the neighbouring arm, of the same or a similar kind to that described in my work on kathodic deflection. This deflection cannot take place through a solid plate.

If now, at high exhaustion, we obtain repeatedly the figure with the dark cross so often mentioned, then, when the plate is introduced, we obtain the figure 19 *b*; or, if the plate does not project so far beyond the kathode, we obtain 19 *c*.

We have therefore, in the first case, only the left bounding

arc of the upper arm of the cross, and only the lower bounding arc of the right-hand arm: the two dark arms themselves cannot be perceived; but the space between the bounding arcs mentioned is uniformly illuminated.

The two bounding arcs, which are now absent, result therefore from an action between the upper and right-hand arms, prevented or hindered by the presence of the screen.

If the plate is withdrawn a little, then, as already mentioned, the figure 19 *c* is seen.

The dark arms which were wanting in the first image are here seen as very narrow strips, and the corresponding two boundary curves are of feeble luminosity. The more the screen is withdrawn towards the kathode the broader do the two dark arms become, and the more closely does the luminosity of the two curves approach to the normal luminosity. The mutual action between each two neighbouring arms, to which the production of the dark cross is due, may be supposed to take place in three different ways :—either (1) as an immediate action of each of the arms of the kathode upon the other, which thus indirectly produces an effect upon the course of the electric rays; or (2) as an action of each arm upon the system of electric rays emitted by the other; or (3) as a mutual action of the two systems of rays.

Any further discussion of the utility of the three hypotheses at present would lead us too far away. I will content myself with remarking that the assumption (1), so far as I see, is opposed to the details of the phenomena of kathodic deflection; but it is not possible to decide certainly between (2) and (3).

Taking account, however, of the fact that the third assumption involves certain accessory assumptions which have not yet been verified by experiment, I shall employ the language of the second hypothesis in seeking for further explanation; which hypothesis, moreover, I have employed throughout in my research on kathodic deflection, in describing the phenomena observed.

If now (fig. 20) rays issue from the edge *a* of the right-hand arm (1) towards *b*, then, according to the laws of kathodic deflection, these rays will be repelled by the edge *b* and the whole surface of (2). The same holds good for the rays emitted by the left arm at *c*. Let fig. 21 be a rough representation of the kathode, turned through 90° from its position in fig. 20, the upper half having moved forwards; and let *l* and *r* represent the upper edges of the left and right arms respectively, and *o* the (fore-shortened) upper arm. The repulsion which *o* exerts upon the systems of rays emitted by *r* and *l* will cause the mutual convergence of these two last

to decrease, but so that the two systems of rays still intersect each other (at x, in fig. 22).

If now a plate capable of phosphorescing move at right angles to the plane of the drawing in the space free from rays between the points of intersection x and o, a dark space will be seen on it, bounded by its luminous intersection with the repelled systems of rays; thus the upper arm of the dark cross is bounded by its two luminous curves. As the density of the gas decreases, the repulsion increases; the convergence of the two pencils of rays will therefore be still further diminished; the point of intersection, x, moves further away from the kathode; and the plate may now be further off from the kathode and still show the dark cross. As we saw already, in examining deflection, the rays further off from the repelling surface are carried with those which are nearer to it, but are not deflected through so great an angle; the deflected rays are therefore compressed together on the side turned towards the repelling surfaces; hence the greater brilliancy of the narrow contour-line immediately bounding the dark arms.

If the phosphorescent plate moves away from o beyond x, the dark cross on it must of course disappear, since the plate comes into a space occupied by rays. We must therefore have a bright field on the plate opposite o, the outer contour of which is again formed by the intersection of the plate with bounding surface of the deflected system of rays.

If we take account of the fact that, according to the form of the dark cross, the curved surfaces of these systems of rays have their *convex* sides turned towards each other before the intersection, we see that beyond x they will have their concave sides towards each other, and will thus form the "curved points," which observation shows to exist.

Consequently the upper "curved point" (fig. 23) is not formed, as we should have expected at first, by rays from the upper arm of the kathode, but it is formed by rays from the two horizontal arms of the kathode—the left-hand half, and in particular the contour-line l, being formed by rays from the right-hand arm R, and the right-hand half by rays from the arm L.

If this is the right way of regarding the curved points, we ought to find it confirmed by experiments in which shadows are thrown. This is, in fact, the case.

If we put a plate as a screen close in front of the kathode, so that, as in fig. 24 a, it covers one half of the kathode obliquely, then at the density at which the curved points appear we observe the phosphorescent figure 24 b. The bounding curves shown in dotted lines are now wanting. The phe-

nomenon thus agrees most exactly with the explanation given above.

If we exhaust up to the density which corresponds to the figure with a dark cross for the uncovered kathode, we obtain the figure 25. By comparison with fig. 24 *b*, we see here also very plainly that the bounding curves of the dark cross, with their convex sides turned towards each other, are nothing else than the bounding lines of a "curved point" with concave sides turned towards each other, which have become displaced across each other.

10. The most remarkable phenomena of the kind we are now considering, however, are undoubtedly those produced by *plane* kathodes.

That plane kathodes cut into figures would present phenomena similar to those of the dark cross was probable, since rays emitted by one edge of an arm of the figure would be repelled by the other edge. Thus, in fact, a four-armed plane (fig. 26 *a*) gives a figure resembling fig. 26 *b*, with a distance of wall from 3 to 4 centim.

The figures 12 *c*, 12 *d*, &c., observed with the curved cross-shaped kathode, do not appear with the plane kathode, but as the density decreases the first figure recognizable is the dark cross; if the density is still further diminished the arms increase in width, and show in the part nearest the centre a nebulous luminosity, with convex contour line (fig. 27).

If the distance of the wall be made less than 3–4 centim., this nebulous portion increases in brightness, and contracts, becoming better defined; and with a distance of wall of about $1\frac{1}{3}$ millim. we obtain the figure represented in fig. 28. In each of the four arms, which would otherwise be dark, there appears a beautifully forked line of light, the space between the forks being filled with uniform light with convex contour, while each fork is connected with the others by a slightly luminous arc of light.

In using a concave spherical cross of the same dimensions as kathode, the radius of curvature being, however, greater than 25 millim., we observe in the arms of the dark cross, so often mentioned, which appears at low pressures, this same luminous fork. We have therefore, as was to be expected, a gradual passage from the forms given by the curved kathodes to those of the plane kathodes, the figures given by the plane kathodes regulating those of the spherical kathodes. Inasmuch as no explanation of the images produced by the former is possible, so for the present no explanation is to be given for a number of phenomena obtained with the curved surfaces. The dark cross produced by the plane cross-shaped kathode

we might foresee; but it is not possible to predict, *à priori*, the phenomena produced by plane kathodes, not cut out, nor having concave edges, and these constitute an entirely new class of phenomena. I make here the general remark that, while it is true that the images given by plane kathodes alter somewhat with decreasing density, they do so much less than the figures obtained with curved kathodes, and often the changes consist only in the acquisition of richer detail and more definite contour, or perhaps somewhat larger dimensions. If these images are produced by kathodes of relatively small surfaces (1 to $1\frac{1}{2}$ square centimetre), the kathodes must be placed tolerably near to the wall of the vessel. The larger the surface of the kathode is, the greater the distance of wall at which it first appears. If with a given kathode we go further off than a certain distance, we obtain a uniform y illuminated phosphorescent surface, whose luminosity slowly and gradually decreases from the centre outwards.

If a plane square be employed as kathode, we obtain fig. 29 on the wall of the spherical discharge-tube of 9 centim. diameter. The relative size and position of the kathode ($2\frac{2}{3}$ centim. distant from the wall) is marked by the dotted lines. We obtain therefore a star of eight arms, four of whose rays correspond in direction to the diagonals of the kathode, and the others to its central lines. The centre of the figure is formed by a feebly illuminated square space, upon which the star appears; a luminous zone surrounds the dark central space, formed of four arcs, convex outwards.

The four rays corresponding to the central lines of the kathodes have their maxima of light within this outer zone; the four diagonal rays, which are narrower than the others, are uniformly luminous, except that all eight rays are more luminous at the centre of the whole figure. The whole figure is considerably larger than the kathode-square, the darker central square space being larger than the kathode.

The figure given by a rectangle 2 centim. by 1 centim., with its longest sides horizontal, on the wall at a distance of $1\frac{1}{3}$ centim., is represented in fig. 30. The main figure is thus a narrow line of light, corresponding to the central line of the rectangle, which forks at each end, and is surrounded by a broad band. The lower ground is an oblong, rounded at the small ends.

A plane circular disk, as mentioned in the introduction, gives no special figure on the illuminated ground obtained in all the figures, unless we regard the bright central point of the image as such a figure. The rays produce a circular disk, which is not sharply defined, with the feebly illuminated ground, and

brighter zone at the edge ; the centre point is, of course, bright. An ellipse whose axes measure 10 mm. and 20 mm., on the other hand, gives a comparatively complicated figure (fig. 31) at a distance of wall of 1 cm. in a vessel of $9\frac{1}{2}$ cm. diameter. In all these figures the ground expands the more the density of the gas is reduced.

If we use kathodes made up of several of these simpler forms joined together, we of course obtain much more complicated images. The images obtained with the comparatively simple form of kathode fig. 32 *a* may serve as example. A square out of whose edges smaller squares have been cut (fig. 32 *b*) shows the central portion of the phosphorescent image completely; but, to save space, the figure gives only two of the streams of light which project from the four sides.

To enter at present further into detail in describing a large number of these phenomena, which are often characterized by surprising beauty, would be without scientific interest, since the simpler cases already described sufficiently represent whatever is new and characteristic amongst phenomena of the kind, viz. :—

(1) The fact that such figures are produced.

(2) The circumstance that the magnitude of the images varies with the density of the gas, and exceeds the magnitude of the kathode itself at high exhaustions.

This latter phenomenon, to which I have devoted a separate series of experiments, may be supposed to occur in either of two ways : either the direction of the rays varies with the change of density, the pencil emitted by a plane becoming more divergent the smaller the density of the gas becomes ; or the direction of the rays remains constant, and as the density of the gas decreases the previously unobserved rays of those elements of the surface which are situated obliquely to the outward-directed rays become strengthened.

Experimental trial gives as result that the first-named reason (variation in direction of rays with variation of density) is to be preferred. Of the different methods of proof employed I will mention only one here.

If we cut slits in a plane disk, the spaces in the disk show themselves in the phosphorescent image, for which the disk acts as a kathode, as narrow dark lines. A number of concentric and equidistant semicircular cuts were made in a disk, so that the outside one lay near the edge of the disk (fig. 33). In the phosphorescent image there appear, even at the highest density at which it is visible, the same number of dark semicircular lines, showing that even at the highest density the phosphorescence produced by the elements on the edge of the

disk is manifest. If the exhaustion be carried further, the dark semicircular lines move further apart; and at a constant density the distance between any two of the dark semicircles is greater the further the pair in question lies from the centre. This last behaviour manifests itself also if the phosphorescent image is received on a plane surface parallel to the kathode instead of upon a spherical wall.

The phenomena described for plane disks lead to the following conclusions:—

(1) The different points of a plane kathode-surface are not of equal value in the emission of kathode-rays, but the intensity of the rays depends on the position of the elements by which they are radiated with respect to the contour-line of the kathode.

(2) The rays of a plane kathode-plate do not in general form a parallel pencil*; but the inclination of the rays varies from element to element, in accordance with the distance from the contour-line of the plate.

(3) The direction of the radiation from each separate element varies moreover with the density of the gas. The smaller the density becomes, the more does the direction of radiation differ from the normal to the element; and the direction of deviation is always outwards.

Whether any density exists at which there would be deviation from the normal in the opposite direction (*i. e.* inwards), at which therefore the rays would be convergent, is a question to which an answer must for the present be deferred. The deviations which make their appearance as the density diminishes are the more considerable the nearer the element in question lies to the edge of the surface.

11. *Convex* kathodes of regular outline also produce regular phosphorescent images.

Convex spherical forms, so far as I have observed, give the same figures as plane kathodes of the same outline—only of larger dimensions at an equal distance of wall, in consequence of the stronger divergence of the rays.

With cylindrical convex surfaces the figures obtained with plane surfaces of similar outline are deformed, as might have been expected; the image is, *cœteris paribus*, more expanded at right angles to the axis of the cylinder than parallel to the

* If we assume that each point of a kathode emits, not simply one ray, but a small conical pencil of rays, then in the above proposition, instead of "ray" we must read "axis of conical pencil of rays." I am still occupied with experiments to determine the limit of aperture which we can ascribe to the pencil of rays from a point of a surface.

axis, and that in greater degree the greater the curvature of the surface.

12. Still another class of simple forms of kathodes which produce figures are such as may be termed "interrupted;" to which belong, amongst others, prismatic tubes open at the ends and cut off at right angles, then (plane) figures formed by bending wire (*e. g.* polygons of wire), and so on.

The images given by such wire kathodes are amongst the most beautiful which can be obtained. In order to render intelligible at least the general mode of their formation, the following may be mentioned:—

If a cylindrical or prismatic tube cut off at right angles to its axis and open at both ends be employed as a kathode, then as the exhaustion proceeds a conical pencil of rays issues from each of the open ends of the tube, the axis of which is coincident with the axis of the tube, and which expands more and more into the gas-space the further the exhaustion is carried. Assuming rectilinear rays, the pencil is then so directed as if it issued from a metal plate closing the actual opening of the tube. When the exhaustion is sufficiently great, this pencil reaches to the wall of the vessel and excites phosphorescence there.

The phosphorescent image of this pencil forms the phosphorescent figure produced by a tube-shaped kathode.

In general, there are two images produced corresponding to the two pencils which issue from the two openings of the tube, and which are congruent if, for example, the discharge-vessel be spherical and the middle point of axis of the tube coincide with the centre of the sphere.

We have a similar result to that obtained with a cylindrical kathode, when the wall of the tube kathode is saddle-shaped, such for example as is formed by the revolution of an arc of a circle about an axis lying on its convex side.

If we imagine such a tube of very small height, we obtain a case which can also be realized by surrounding a space with a wire of the corresponding form. Just as pencils of rays issued at right angles to the opening of the tube, so they issue from the wire kathode at right angles to its plane, to all appearance as if the empty space surrounded by the wire acted as a kathode.

The luminous figures obtained from wire kathodes are larger than the space enclosed by the wire, even at small distances of the wall from the kathode. The images may show great changes with change of pressure. I content myself with

giving as example the image represented in fig. 34 *b*, given at high exhaustion on the wall of a spherical vessel by a regular pentagon of 12 millim. in the side bent out of wire about 1 millim. thick (fig. 34 *a*). The normal to the polygon at its centre point was placed radially.

The waving contour-line which passes through the ends of the five-rayed star forms a perfectly sharply defined contour-line.

If we employ a regular wire polygon of some other number of sides (3–8), we obtain a similar luminous star with the corresponding number of rays. With polygons with an odd number of sides, the rays of the star correspond in direction to the longer radii of the kathode-polygon.

With polygons of an even number of sides, on the other hand, the axes of the rays correspond to the shorter radii of the polygon, and thus appear to intersect the sides of the polygon at right angles.

With the phenomena which are given by interrupted kathodes may be connected an observation made with plates perforated with holes, which is at first sight surprising. If we make use of a square plate perforated with a number of holes (fig. 35), we might perhaps expect that the places from which the metal has been removed would appear dark in the phosphorescent images, or at least would correspond to mimima of light. We find, however, that these points are really maxima of light; thus with the kathode fig. 35 we obtain 16 very bright points of light. The reason is to be found in the fact that the walls of each perforation form a short open tube; and, according to what we have seen, a bright pencil of light issues from such a tube, which at suitable exhaustion extends to the wall and excites phosphorescence there.

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LII. *Notices respecting New Books.*

Graphical Determination of Forces in Engineering Structures. By JAMES B. CHALMERS, *C.E.* London: Macmillan. 1881. 405+xxvi pages, 6 plates, 267 cuts.

THIS is a large and important work, aiming at being a complete treatise on use of graphic methods in engineering-designs. It is a high-class work, requiring a fair knowledge of modern geometry for its comprehension. To facilitate this (to the Engineer) a special Chapter on "Projective Geometry" (66 pages) is given; within this compass a wide range is compressed, *e. g.* projections, homology, Carnot's, Pascal's, Desargues's theorems, &c.

In application to Engineering great superiority is claimed for graphic methods over computation. The practical applications are skilfully and neatly worked out; and the study of them is an intellectual treat (not easy reading). The scope of the work is very wide: Resultants of Forces, Moments, Centre of Gravity, Moments of Inertia, and Stresses in Structures, *e.g.* in Frames, Beams (supported, fixed, and continuous), Arches (rigid and elastic), Suspension-Bridges, Retaining Walls, and Tunnels are all treated by graphic methods. These processes are only meant to supersede computation: analysis is often used for their actual elucidation; thus the explanation of the graphic methods for the Elastic Arch covers 20 pages of a somewhat difficult analysis.

In some cases the graphic methods have decided advantage, chiefly when the work is simply a repeated application of the theorem of the "polygon of forces;" the diagrams of these are simple and can be quickly drawn. But in cases where sums of products are required (*e. g.* in moments of forces, moments of inertia, &c.), the advantage is not so clear: the process increases greatly in complexity as the number of variable factors in each product increases, the diagrams become intricate, and are finally a network of lines (see fig. 65–i, fig. 143 pl. I, pl. V a), requiring great skill in their original preparation, and not to be unravelled in after-examination without careful study. The risk of mistake in construction, not so much from inaccurate drawing as from mistaking one point or line for another, in such a network must be considerable, and quite analogous to that of numerical slips in computing. Even in the simple case of the sum of the products of two factors, the result would probably be got more quickly with a slide-rule or Crelle's mutiplication-table than by the very neat graphic process given; but in the more complex cases of several factors computation would surely be quicker.

The processes given are by no means always the shortest. Thus the determination of the pressures on the supports due to a single load placed on a beam requires only the division of the line representative of the load into segments inversely proportional to the segments of the beam on either side of the load. This requires only three lines for its complete graphic solution; but the process given (the same as for the general case of many loads) requires seven lines.

When several processes are available for the same purpose, it would surely suffice to give the best, unless each has peculiar advantages in special cases, which should then be stated. Now, for finding stresses in frameworks three processes are given, covering 66 pages. No. i., by "Method of Sections," requiring at every section a preliminary reduction of the frame on one side thereof to an ideal triangle and evaluation of the Resultant force thereon: this is a difficult and troublesome process. This troublesome preliminary is avoided in No. ii., the Method of Sections proper: this is nearly the same in its application as No. iii., but gives rise to

stress-diagrams sometimes imperfectly "reciprocal" to the original —a slight disadvantage. No. iii. is Clerk-Maxwell's beautiful process: this is the simplest and easiest of the three; its simplicity seems to depend on the complete reciprocity of the stress-diagram with the original figure. Methods i. and ii. might have been omitted with advantage, and more space given to the last. This Chapter is illustrated by numerous* well-chosen examples. The three processes therein are really a graphic solution of the "conditions of equilibrium" among the forces at each section or joint; as there are thus only two equations for each section or joint, the magnitudes of two stresses can be found for each section or joint. Thus the problem is indeterminate for a frame at any of whose joints so many bars meet as to require the determination of more than two stresses thereat. This is actually the case with two of the frames (figs. 49 and 51) for which finished stress-diagrams are given without comment. Some explanation is surely wanted in the text as to how this indeterminateness (which is inherent in both) is to be met. One of these (No. 51) is solved in Rankine's 'Civil Engineering,' art. 576, by a method of dissecting the complex Truss into partial Trusses, which bridges the difficulty by (tacitly) assuming the interaction of the partial Trusses.

In Clerk-Maxwell's process for Frames under dead load the graphic methods probably appear at their best; but with moving load the greatest stress in each bar occurs with a different state of load, thus involving a tolerably complete special diagram for each bar, greatly increasing the work and the intricacy of the finished drawing.

In investigating the stability of Retaining Walls and Masonry Arches, again, the graphic methods have decided advantage over computation: this arises partly from the fact of the cross-sections being solid, so that the limit within which the centre of pressure at each joint should fall is easily known to be the middle third. The tracing of lines of pressure and resistance therein is well explained and illustrated.

In the case of the Arch, however, one difficulty (indeterminateness) has not been adequately met. In general many lines of pressure and resistance could perhaps be traced within the "core" or admissible limits (the middle third); and the question is, which is the true line? The author says, "the true line of pressures is that which is nearest the axial line" (art. 181); this seems doubtful. Moseley's Principle of Least Resistance gives a means of locating it so that the passive resistance required at the springing shall be the least: this seems sound for rigid material; but its applicability to non-rigid material is not so clear.

Of all the processes given, the applications to Continuous Beams and the Elastic Arch are naturally the most intricate. These are masterly specimens of the power of graphic work in the hands of

* There are several lines wrong in lower part of fig. 57 *b*.

one skilled in its use. The amount of drawing required for the complete investigation of any one arch seems very great. Pl. v. a, the finished (?) result for an arch, is so complex a whole (although several preliminary drawings are omitted from it) as to require great care for its comprehension; and even it seems (arts. 214, 219) to be only a part of what is required.

Among practical details, it is laid down (art. 121) that "an arch ought to be wholly in compression." Now this principle is obviously right for masonry; but there can be no occasion for applying it to iron or steel arches (as in art. 183): this would surely be a waste of power.

The theory of earth-pressure given, depending on the "angle of repose" and frictional stability, is complex and difficult (covering 23 pages before application to retaining walls). The "angle of repose" is an item which in many cases can hardly be said to be known at all, so that mathematical refinements are of little use. Rankine's theory (which is much simpler) seems good enough for such imperfect data.

There are numerous references to foreign works on geometry and graphic statics; the influence of these is obvious in the diction. The author is thoroughly at home in the practical application of graphic methods; but for a didactic work the mathematical rendering might be improved. Thus there is occasional obscurity in the explanations, *e. g.* props. xxxix., xlii., and arts. 80, 161, 223: results to be derived as the fourth term of a proportion are commonly presented as $\left(a : b :: c : \frac{bc}{a}\right)$ a mere identity; the insertion of the name or symbol for the required fourth term would be more useful (*e. g.* in aiding its discovery in the diagrams). There is also a certain looseness of expression, *e. g.* moments termed forces (pp. 113, 139), the use of the term "centre of gravity" of forces (pp. 268, 274): also of notation, *e. g.* in use of symbols Δ and d, Σ and $\int$ (*passim*), and of $-$ in geometry (pp. 363, 364); also of analysis, *e. g.* omission, removal, or change of variables under summatory symbol (pp. 162, 268; 150, 275, 277; 281); these latter mistakes generally correct themselves in the final results. There are also two mistakes in the geometric theorems. Thus, Poncelet's condition of projectivity (prop. xxxii.) is stated in too general terms without due limitations, and the example given is non-projective. Again, in the proof of Pascal's theorem (prop. xlvi.), the conic and its inscribed hexagon are projected into a circle and inscribed hexagon with opposite sides parallel and one pair equal (which is not generally possible); and it is stated that "the points of the hexagon joined two and two concur in a point P" (and they actually do in the figure in consequence of its being a regular hexagon), which is not generally true.

Several minor points might be improved (in a new edition). Thus there is hardly enough lettering on some of the diagrams for their easy comprehension; and in many cases the symbols given are

too complex for use on a diagram—*e. g.* one length in fig. 100 is marked thus:

$$\frac{\dot{A}_1}{a} \cdot z_1 \cdot \left(z + \frac{k^2_1}{z_1}\right) : c \cdot \tfrac{1}{2}\,\Sigma \frac{\dot{A}}{a}.$$

Now in all such cases a single symbol (with reference in the text) would be better. The numbering of the diagrams also should be made consecutive; there are at present three numberings intermixed, which renders reference difficult. The number of misprints also is very great, especially among the references. Even with these faults the work is a valuable one, and no one can read it without learning much. ALLAN CUNNINGHAM, *Major R.E.*

Questions in Pure Mathematics proposed at the B.A. and B.Sc. Pass and Honours Examinations of the University of London, with complete Solutions by J. E. A. STEGGALL, *M.A.* Van Voorst, 1882; pp. viii+245.

THE title sufficiently indicates the nature of the work. The solutions, we think, are in all cases neat, and in many instances they are elegant. Mr. Steggall does not confine himself to single solutions, but often gives two or more proofs of the same question. The work is very carefully printed, and there are, we believe, very few typographical errors. On p. 13 another mode of solution might have been indicated, depending on the fact that the sinister side vanishes when $x=y=z$. We venture to suggest that on p. 25 reference might have been made to Euc. vi. 3 and A as also readily furnishing a solution; and the equation on p. 37 might be worked from the fact that the terms on one side are reciprocals of those on the other. But these and other instances we could bring forward only illustrate the well-known fact that there are more ways than one of attacking problems; and the exigencies of space have no doubt restricted the author in general to the single solution he adopts. On pp. 21, 45, occur, as we think, two slight inaccuracies in expression. We note the following slips:—p. 120, "Solving for y" read "x," and for "$\cos x$" read "$\cos^2 x$"; p. 164, for "$-3y$" read "$+3y$"; p. 224, for ">1" read "<1." A few others are easily corrected; but on p. 205 there is a great derangement of "subscripts," and this may puzzle some readers. We hope that Mr. Steggall will receive sufficient encouragement to bring out a second edition in a few years time, with additional solutions up to date; for such works as this are of great service to students.

Geological Chart, arranged by Professor JOHN MORRIS, *M.A., F.G.S., &c.* New Edition. Large Sheet. Reynolds and Sons, Strand, London. 1882.

THIS enlarged and revised edition of a good Geological Table shows the order of the many stratified formations in their regular succession, their mineral characters, uses in the arts, principal fossils,

places of occurrence in the British Isles, and their relative thicknesses. Notes also, to similar effect, on the Metamorphic and Igneous rocks are supplied. The later determinations of geologists as to the better division and classification of some of the recognized groups of strata are, in several instances, incorporated where they are not likely to be otherwise than clear and useful to the student; and there are but few points in printing or arrangement which we would find fault with. We therefore recommend this Chart as having been carefully revised by its well-known accomplished author, and as having been brought up to the latest date in useful information, and forming a complete and ready Geological Synopsis for the several lines of study indicated above.

LIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 151.]

November 1, 1882.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Hornblendic and other Schists of the Lizard District, with some Additional Notes on the Serpentine." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

The author described the metamorphic series, chiefly characterized by hornblendic schist, which occupies the southern portion of the Lizard and an extensive tract to the north of the serpentine region, besides some more limited areas. He found that this series was separable into a lower or micaceous group—schists with various green minerals (often a variety of hornblende), or with brownish mica; a middle or hornblendic group, characterized by black hornblende; and an upper or granulitic group, characterized by bands of quartz-felspar rock, often resembling in appearance a vein-granite. These are all highly metamorphosed; yet the second and third occasionally retain to a remarkable extent indications of the minuter bedding structures, such as alternating lamination and current-bedding of various kinds. They form, in the author's opinion, one continuous series, of which the uppermost is the thinnest. The general strike of the series, though there are many variations, is either N.W. or W.N.W.

The junctions of the Palæozoic with the metamorphic series at Polurrian and at Porthalla were described. These are undoubtedly faulted; and the two rocks differ greatly, the former being a slate like any ordinary Palæozoic rock, the other a highly metamorphosed schist. Moreover fragments of the hornblende schist and a kind of gneiss occur in a conglomerate in the former, S. of Nare Point.

The author considers the metamorphic series (the microscopic

structure of which was fully described) undoubtedly Archæan, and probably rather early in that division. The rocks of the micaceous group have considerable resemblance to the greenish and lead-coloured schists of Holyhead Island and the adjoining mainland of Anglesey, and of the Menai Strait.

Two outlying areas of serpentine, omitted in his former paper, were described—one at Polkerris, the other at Porthalla. The latter shows excellent junctions, and is clearly intrusive in the schist. The author stated that he had reexamined a large part of the district described in his former paper, and had obtained additional evidence of the intrusion of the serpentine into the sedimentary rock with which it is associated. This evidence is of so strong a nature that he could not conceive the possibility of any one who would carefully examine the district for himself entertaining a doubt upon the matter.

2. "Notes on some Upper Jurassic Astrorhizidæ and Lituolidæ." By Dr. Rudolf Häusler, F.G.S.

LIV. *Intelligence and Miscellaneous Articles.*

ON MR. C. W. SIEMENS'S NEW THEORY OF THE SUN. BY M. G. A. HIRN.

TO the grave objection brought forward by M. Faye against Mr. Siemens's new theory of the conservation of the solar energy, another, also a very serious one, may be added. This objection may be summed up in few words.

Up to the present time there is no general agreement as to the real value of the Sun's temperature. Père Secchi carried it to millions of degrees. Other physicists, especially in France, lowered it to about twenty thousand degrees. According to the magnificent experiments of Mr. Langley (of Alleghany) this latter amount is, at any rate, a minimum. What is certain then, starting from the fine memoirs upon dissociation of our lamented colleague Henri Sainte-Claire Deville, is that none of the chemical compounds that we know upon our Earth could exist at the surface of the Sun. All, even those which are most refractory in our laboratories, would be dissociated and reduced to their constituent elements. And this is what is admitted in M. Faye's theory of the Sun.

The natural and direct consequence of the preceding fact is, that the chemical compounds which Mr. Siemens supposes to be dissociated by degrees in space by the solar radiation, might certainly, in returning under the action of gravity and in the elementary state towards the central body, become reformed, and regenerate the heat expended in their dissociation in space; but this recombination could only be effected at an appreciable distance from the solar photosphere, and the compounds reproduced, on falling into the bosom of the latter, would be again completely dissociated. This action, therefore, would cause the expenditure of all the heat

previously developed by the combination. From this it follows evidently that this return of the elements towards the centre would contribute nothing at all towards the conservation, or rather the continuous reproduction, of the solar temperature.

It seems to me that Mr. Siemens's theory may be subjected to another decisive critical test. If the solar radiation, or say the heat, whether visible or not, emitted or sent off by any celestial body, during its course effects the chemical dissociation of the hypothetical compounds disseminated in stellar space, the intensity of this radiation must necessarily be reduced by the positive work effected, and all that serves for this work is lost for the visibility of the star.

From this, then, it follows that the lustre of the sun, of the stars, and of the planets must diminish according to a *much more rapid* law than that of the inverse proportion of the square of the distances. I say *much more rapid*; but we must say *extremely rapid*. In fact, from the moment when the recombination of the elements at the surface of the Sun would be capable of regenerating the heat emitted, it is evident that all this emitted heat would be employed in its turn in dissociating the chemical compounds in space. In order that the Sun could be thus continually maintained in its energy, it would be necessary that the distance at which it is visible, far from being unlimited as it probably is, should, on the contrary, be restricted; for wherever it would be still visible there would be light *not employed in chemical dissociation*, and consequently there would still be a definite loss *possible*. Nothing in the aspect of our planets and their satellites, it seems to me, authorizes us to assume that there is any other reduction in the brilliancy of the light than that resulting from the inverse proportion of the square of their distance from the central body. We see stars the light of which has taken at least three years, and others of which the light has perhaps taken thousands of years to reach us. None of this light, therefore, has been employed in chemical dissociation; nothing could have been restored to them by the mode indicated by the ingenious theory of Mr. Siemens.

May I be permitted, in concluding this note, to revert to the objection formulated by M. Faye, and to render it in some degree palpable by a numerical example? In an extensive work upon which I am engaged, upon the constitution of the stellar space, I naturally examine into the consequences that the resistance of a gas diffused in space would have upon the movements of the planets. From this work I extract an example relating to the application of analysis to the motion of our Earth. According to Laplace, the diminution or augmentation which one may attribute to the duration of our sidereal year 3000 years ago, taking into account the uncertainty of the observations, would be 90 seconds at the maximum (a modification of which, however, there is nothing to demonstrate the reality). Accepting a *reduction* of this amount as real, I inquire what density a gas would need to have to produce

it; and I show that it would suffice if there were 1 kilogr. of matter in vapour in 700 thousand millions of cubic metres—in other words, that the density would be 0·000 000 000 001 43 kilogr. It will be seen that we are far from the reduction to the $\frac{1}{2000}$, and even to the millionth assumed by Mr. Siemens. If, instead of taking account only of the resistance opposed by such a gas to the motion of our planet, we direct our attention to the consequences which its existence would have upon that of our atmosphere, we find that, unless we multiply our 700 thousand millions of cubic metres by 10,000, and reduce the density sought for to 0·000 000 000 000 0001 kilogr., our atmosphere would be in a few moments *swept away* by the pressure exerted above by the interstellar gas.

M. Faye is perfectly justified in saying that it is not such or such a degree of rarefaction, but that it is the vacuum (*of matter*, of course) that the astronomer requires to ensure the stability of the movements that his analysis shows. This vacuum no doubt upsets the doctrine, supposed to be so undeniable, which ascribes all the phenomena of the physical world to movements and collisions of material atoms independent of each other. One day or another, no doubt, this doctrine will have to give up its existence, and its defenders will have to resign themselves to admit in the physical world something more than matter in motion. In a remarkable letter to Bentley, Newton said that one must be destitute of all aptitude for a serious philosophical discussion to suppose that between two bodies which seem to attract each other at an unlimited distance, there is not something which establishes this relation; but, he adds immediately, is this intermediary material or immaterial? This I leave to the reader to decide. With that great genius undoubtedly there was no uncertainty upon this latter point; but, perhaps justly, he refrained from putting before his contemporaries a solution which might have seemed incomprehensible to them, as it still is, apparently, to so many minds of the present day.—*Comptes Rendus*, November 6, 1882, p. 812.

REPLY TO M. FAYE'S OBJECTIONS TO MR. C. W. SIEMENS'S THEORY OF THE SUN. BY C. W. SIEMENS.

M. Faye, while approving, generally, of the physical part of my investigations, questions their application to astronomy, and for the following reasons:—

1. That the presence of a universal gaseous medium at a pressure of $\frac{1}{2000}$ atmosphere would oppose an excessive resistance to the movements of the planets; 2. That this vapour, thus distributed, would be gradually attracted towards the sun and would tend to augment its mass considerably.

Allow me to point out, as regards, in the first place, the second of M. Faye's objections, that the degree of diffusion supposed by me is such as may ensure the permanence of the statical equilibrium between the forces of expansion and diffusion on the one hand, and

the attraction towards the sun and the celestial bodies on the other. If no such equilibrium were established, M. Faye's objection would at once upset my theory. I am, moreover, inclined to admit that if Mariotte's law with regard to the tension of gases could be applied indefinitely, the pressure of the interplanetary gaseous medium would be reduced almost beyond any thing of which we can form an idea; but it seems to me, from considerations drawn from the dynamical theory of gases, and from the manner in which, as demonstrated by Mr. Crookes, gases behave when rarefied to an extraordinary degree in tubes—it seems to me, I say, that at least there exists no reason *à priori* why this law should be extended rigorously to vapours beyond the confines of our atmosphere and of that of the Sun.

As regards M. Faye's first objection, I admit that a density of $\frac{1}{2000}$ atmosphere would have the consequences which he so correctly establishes; and I remember having said (see 'Proceedings of the Royal Society,' p. 395) that assuming as demonstrated the results of my experiments on the dissociation of vapours by the solar energy, and that stellar space is filled with vapour at a pressure *not exceeding* the limit of $\frac{1}{1000}$ atmosphere, which corresponds to the highest rarefaction that I was able to obtain in my experiments, a dissociation of this cosmical vapour must ensue by the radiation of the Sun. It must nevertheless be remarked that this observation only relates to the physical phenomena submitted to my experiments, and that it is evident that if, the dissociation of aqueous vapour and of carbon-compounds is effected by the direct radiation of the sun at so high a pressure as $\frac{1}{1000}$ atmosphere, it would with still more reason be effected in the much more rarefied medium.

In another passage of my memoir (p. 397), when I apply my hypothesis to comets, I assume that, even at their perihelion, they represent a vapour-medium with a density of only $\frac{1}{3000}$ atmosphere, and that this density suffices to give rise to incandescence by compression. This supposition proves, at any rate indirectly, that I regarded stellar space as filled with vapour at a pressure much below $\frac{1}{3000}$ atmosphere, while still speaking of this medium (in the absence of all data of experiment and observation) as in an extremely rarefied state, without fixing any limit of this rarefaction.

Since then new facts of observation have tended to confirm my hypothesis of a stellar space filled with rarefied matter analogous to that which we can actually produce in our vacuum-tubes. The equatorial prolongations of the solar atmosphere observed in America during the eclipse of 1859, seem to demonstrate the existence of matter extending from the Sun for several millions of leagues, and rendered visible, no doubt, by solid particles, illuminated partly by the reflection of the solar light, and partly by discharges of electricity towards the Sun.

My hypothesis has found a still more direct confirmation in the remarkable spectroscopic investigations communicated by Capt. Abney to Section A of the British Association in the month of August last, which demonstrate that carbon-compounds, probably

analogous to ethyl, easy to observe distinctly, exist, and at a low temperature, between the Sun's atmosphere and our own. The observations made in America by Prof. Langley with his bolometer, although made for a totally different purpose, tend to confirm the results obtained by Capt. Abney upon the Riffel. We may also add to these proofs the interesting observation of Prof. Schwedoff (still unpublished, and communicated to me on the same occasion by Prof. Silvanus Thompson), according to which large hailstones of cosmical origin have sometimes fallen upon the earth. This observation, however, needs to be confirmed.

Accepting these observations as founded upon facts, physical considerations are not wanting for the approximate determination of the actual density of the stellar vapour, which, in this case, is only a function of the temperature of space. As Gorschow, on 30th November 1871, observed a temperature of $-63°$ C. in the Arctic regions, it follows that the stellar medium (which, if it consists of a vapour, must be able to intercept calorific rays) must be at a temperature comprised between $-63°$ and the absolute zero ($-273°$); the solar radiation must maintain in it some temperature, or, at least, such a temperature that the dissociation of this medium is very active.

It is to Regnault that we owe our most exact knowledge of the density of vapours at different temperatures; but his researches did not extend below $-32°$ C., and his formulæ cannot be rigorously applied below that point; nevertheless they enable us to estimate approximately what may be the densities of a vapour at lower temperatures; and it is thus that we are led to believe that at $-130°$ the density of aqueous vapour does not exceed $\frac{1}{5,000,000}$ atmosphere. If we assume, further, that the gaseous mass which fills space contains only $\frac{1}{5}$ of aqueous vapour, the other four fifths being composed of hydrocarbons, carbonic acid, and nitrogen, the total pressure of the vapour would not exceed $\frac{1}{1,000,000}$ atmosphere.

These vapours would traverse space with a velocity equal, probably, to half the tangential velocity at the surface of the Sun, or at about 1 kilometre per second. It would be easily demonstrated that a column of these dissociated gases travelling with this velocity towards the polar surfaces of the sun and taken at a distance of 5,500,000 kilom. from the Sun (equal to the mean distance of Mercury, the nearest of his planets) would present a section of flow towards the Sun equal to 140,000 milliards of square kilometres, much more than sufficient to furnish the material necessary to yield by combustion the heat required to maintain the solar radiation.

Perhaps the eminent Director of the Bureau des Longitudes may be inclined to think that a gaseous medium of a density equal at most to $\frac{1}{1,000,000}$ of that of our atmosphere might still interfere with planetary movements to a degree incompatible with the facts ascertained by astronomical observations. If this be the case, it would suffice to assume a still lower temperature for this medium,

and in consequence a more attenuated rarefaction of the interstellar gaseous matter.—*Comptes Rendus de l'Académie des Sciences*, Oct. 30, 1882, p. 769.

ON A PROPERTY OF THE COEFFICIENT OF ABSORPTION. BY EILHARD WIEDEMANN.

Bunsen represents the absorption-coefficient α at a temperature t by the formula

$$\alpha=a-bt+ct^2,$$

in which a and $b=$ a constant.

Instead of this, we can write

$$\alpha=a\left\{1-\frac{b}{a}t+\frac{c}{a}t^2\right\}$$

In the following Table I have set down, together with the values of a, $b\,.\,10^6$, and $c\,.\,10^7$, the values of b/a and c/a for a series of gases and water:—

	a.	b.	c.	$\frac{b}{a}$.	$\frac{c}{a}$.
Hydrogen	0·0193	0	0	0	0
Nitrogen	0·0203	539	112	0·02648	0·000548
Air	0·0247	654	135	0·026487	0·000548
Diethyl	0·0315	1045	251	0·033198	0·000796
Carbonic oxide	0·0329	816	164	0·0243	0·000499
Oxygen	0·0412	1089	226	0·02648	0·000548
Marsh-gas	0·0545	1180	103	0·02166	0·000188
Dimethyl	0·0871	3324	603	0·03816	0·000692
Hydride of ethyl	0·0946	3532	628	0·03735	0·000663
Ethylene	0·2563	9136	1881	0·03564	0·000654
Propylene	0·4465	22075	5388	0·04943	0·000206
Nitrous oxide	1·3052	45362	6483	0·03475	0·000496
Carbonic acid	1·7967	77610	16424	0·04320	0·000914
Sulphuretted hydrogen	4·3706	83687	5213	0·01914	0·000119
Sulphurous acid	79·789	2607700	263490	0·03268	0·000367
Chlorine	3·0361	46196	1107		

It will be remarked that the values of b/a in this Table vary only from 0·02 to 0·05, while a itself ascends from 0·02 to 79·789, *i. e.* up to 4000 times the former value. But the values of b/a indicate how large a fraction of the gas absorbed at 0° escapes on the temperature being raised 1°; and from the above it follows that that fraction varies within very narrow limits for all gases.

In order to further test this proposition, it would be necessary to institute experiments for larger intervals of temperature.

A comparison of the absorptions in alcohol gave similar results. —Wiedemann's *Annalen*, vol. xvii. p. 349 (1882).

INDEX TO VOL. XIV.

ABSORPTION, on a property of the coefficient of, 483.
Accumulators, on the construction and charging of, 249.
Aerial vibrations, on an instrument capable of measuring the intensity of, 186.
Air-thermometer, on an, 233.
Amagat (E. H.) on the elasticity of rarefied gases, 403.
Anemometer, on an integrating, 212.
Aniline, on the melting-points of some derivatives of, 6.
Atmosphere, on carbon dioxide as a constituent of the, 387.
Atom, on the motion of a spherical, in an ideal gas, 157.
Attwood (G.) on the geology of Costa Rica, 141.
Ayrton (Prof. W. E.) on the Faure accumulator, 41; on a simplified dispersion-photometer, 45.
Babo (L. v.) on the connexion between viscosity and density in fluids, 51.
Baily (W.) on an integrating anemometer, 212.
Barometric pressure, on the mechanical effects of, on the earth's surface, 409.
Battery, on the oscillations of the plane of polarization produced by the discharge of a, 79.
Benzol, on the melting-points of some derivatives of, 2.
Bevan (E. J.) on the correlation of the chemistry of the carbon compounds with the phenomena of life, 346.
Bichat (D.) on the oscillations of the plane of polarization produced by the discharge of a battery, 79.
Blondlot (R.) on the oscillations of the plane of polarization produced by the discharge of a battery, 79.
Boltzmann (Prof.) on the average distribution of energy in a system of material points, 299.
Bonney (Prof. T. G.) on the Hornblendic and other schists of the Lizard district, 477.
Books, new:—Selwyn's Geological and Natural History Survey of Canada, 70; Geikie's Geological Sketches, 71; Clifford's Mathematical Papers, 135; Muir's Theory of Determinants, 140; Lunge's Distillation of Coal-tar, 228; Hulme's Examination Questions in Plane Geometrical Drawing, 230; Latimer Clark's Treatise on the Transit Instrument, 319; Geology of Wisconsin, 319; Stuckenberg's Life of Immanuel Kant, 322; Stallo's Concepts and Theories of Modern Physics, 396; Chalmers's Graphical Determination of Forces in Engineering Structures, 472; Steggall's Questions in Pure Mathematics, 476; Prof. Morris's Geological Chart, 476.
Bosanquet (R. H. M.), notes on practical electricity by, 241.
Boys (C. V.) on the measurement of curvature and refractive index, 30.

Brongersma (H.) on double refraction produced by electrical influence in glass and bisulphide of carbon, 127.
Brown (F. D.) on thermometry, 57.
Carbon, on double refraction produced by electrical influence in bisulphide of, 127; on the appearances of the electric arc in the vapour of the bisulphide of, 324.
—— compounds, on the correlation of the chemistry of the, with the phenomena of life, 346.
Carbonic acid, on the viscosity of liquid, 55; on the surface-tension of some liquids in contact with, 327.
Charpentier (A.) on the duration of the perception of light in direct and indirect vision, 230.
Chase (Dr. P. E.) on the conservation of solar energy, 322.
Chemical affinity, on the determination of, in terms of electromotive force, 188.
Chemistry of the carbon compounds, on the correlation of the, with the phenomena of life, 346.
Clausius (Prof. R.) on the dimensions of a unit of magnetism in the electrostatic system of measures, 124.
Comets, on the tails of, 292.
Cook (E. H.) on carbon dioxide as a constituent of the atmosphere, 387.
Copper pyrites, on twins of, 276.
Crafts (J.-M.) on the depression of the zero-point in mercurial thermometers, 76.
Cross (C. F.) on the correlation of the chemistry of the carbon compounds with the phenomena of life, 346.
Crystallographic notes, 119, 276.
Cunningham (Major A.) on Moseley's theory of steady flow, 110.
Curvature, on the measurement of, 30.
Darwin (G. H.) on variations in the vertical due to elasticity of the earth's surface, 409.
Debray (H.) on some explosive alloys of zinc and the platinum metals, 152.
Deville (H. Sainte-Claire) on some explosive alloys of zinc and the platinum metals, 152.
Dunstan (W. R.) on science and metaphysic, 75.
Earth, on the physics of the crust of the, 213; on variations in the vertical due to elasticity of the surface of the, 409.
Earth-moon system, on Darwin's theory of the, 427.
Edison's tasimeter, on the influence of time on the change in the resistance of the carbon disk of, 115.
Electric and magnetic quantities, on systems of absolute measures for, 430.
—— arc, on the reaction-current of the, 154; on the appearances of the, in the vapour of bisulphide of carbon, 324.
—— discharge in rarefied gases, on the, 366.
—— resistance of glass at low temperatures, on the, 325.
—— spark, on vision by the light of the, 313.
Electrical influence, on double refraction produced by, 127.
—— rays, on the reflection of, 449.
Electricity of flame, on the, 161; on the equilibrium of liquid conducting masses charged with, 184; notes on practical, 241.
Electromotive force, on the determination of chemical affinity in terms of, 188.
Electrostatic system of units, on the dimensions of a magnetic pole in the, 124, 225, 357, 395.
Elster (J.) on the electricity of flame, 161.
Energy, on the average distribution of, in a system of material points, 299.
Faure accumulator, experiments on the, 41.
Faye (M.) on Dr. Siemens's new theory of the sun, 400.
Fisher (Rev. O.) on the effect upon the ocean-tides of a liquid substratum beneath the earth's crust, 213.
Flame, on the electricity of, 161.
Fletcher (L.), crystallographic notes by, 276.
Fluids, on the connexion between viscosity and density in, 51.

Forel (F.-A.) on the structure and movement of glaciers, 238.
Foussereau (G.) on the electric resistance of glass at low temperatures, 325.
Gas, on a property of the isentropic curve for a perfect, 233.
Gaseous fluids, on the connexion between viscosity and density in, 51.
Gases, on the electric discharge in rarefied, 366; on the elasticity of rarefied, 403.
Geissler tubes, on the connexion between the gas-density and stratum-interval in, 402; on the distribution of phosphorescent light in, 455.
Geitel (H.) on the electricity of flame, 161.
Geological Society, proceedings of the, 73, 141, 477.
—— time, on the duration of, 427.
Glacial period, on the, 151.
Glaciers, on the structure and movement of, 238.
Glass, on double refraction produced by electrical influence in, 127; on the electric resistance of, 325.
Goldstein (Dr. E.) on the electric discharge in rarefied gases, 366; on the connexion between the gas-density and stratum-interval in Geissler tubes, 402; on the reflection of electrical rays, 449; on the influence of the shape of the kathode on the distribution of phosphorescent light in Geissler tubes, 455.
Haughton (Rev. S.) on Darwin's theory of the evolution of the earth-moon system, 427.
Helmholtz (Prof. H.) on systems of absolute measures for electric and magnetic quantities, 430.
Hirn (G. A.) on Siemens's new theory of the sun, 478.
Holman (S. W.) on a simple method for calibrating thermometers, 294.
Idocrase, on the crystalline form of, 121.
Isentropic curve, on a property of the, for a perfect gas, 233.
Jamieson (T. F.) on the cause of the depression and re-elevation of the land during the glacial period, 151.
Jamin (M.) on the reaction-current of the electric arc, 154; on the appearances of the electric arc in the vapour of bisulphide of carbon, 324.
Johnston-Lavis (H. J.) on the comparative specific gravities of molten and solidified Vesuvian lavas, 141.
Judd (Prof. J. W.) on the relations of the eocene and oligocene strata in the Hampshire Basin, 73.
Kerr's phenomena, observations on, 127.
Lapworth (Prof. C.) on the Girvan succession, 147.
Lava, on the specific gravities of molten and solidified Vesuvian, 141.
Lewis (W. J.), crystallographic notes by, 119.
Life, on the correlation of the chemistry of the carbon compounds with the phenomena of, 346.
Light, on the duration of the perception of, 230; on the influence of the shape of the kathode on the distribution of phosphorescent, in Geissler tubes, 455.
Lippmann (G.) on a thermoscopic method for the determination of the ohm, 407.
Liquid, on the steady flow of a, 110; on the influence of the quantity of gas dissolved in a, on its surface-tension, 236; on the surface-tension of a, in contact with carbonic acid, 327.
Lodge (Dr. O. J.) on the dimensions of a magnetic pole in the electrostatic system of units, 357.
Ludlamite, on the crystalline form of, 120.
Lübeck (G.) on the motion of a spherical atom in an ideal gas, 157.
Magnetic and electric quantities, on systems of absolute measures for, 430.
Magnetism, on the dimensions of a unit of, in the electrostatic system of measures, 124, 225, 357, 395.
Magnetometer, on a, 227.
Maneuvrier (G.) on the reaction-current of the electric arc, 154; on the appearances of the electric arc in the vapour of bisulphide of carbon, 324.

Melting-point, researches on, 1.
Mendenhall (T. C.) on Edison's tasimeter, 115.
Metalloids, on the influence of temperature upon the spectra of, 406.
Michelson (A. A.) an an air-thermometer, 233.
Mills (Dr. E. J.) on melting-point, 1.
Monckhoven (M. D. van) on the influence of temperature upon the spectra of metalloids, 406.
Moseley's theory of steady flow, observations on, 110.
Naphthalin, on the melting-point of derivatives of, 25.
Neale (E. V.) on the tails of comets, 292.
Nipher (F. E.) on a property of the isentropic curve for a perfect gas, 233.
Ocean-tides, on the effect upon the, of a liquid substratum beneath the earth's crust, 213.
Ohm, on the methods employed for determining the, 258, 329; on a thermoscopic method for the determination of the, 407.
Optics, notes on physiological, 312.
Penrose (C. B.) on the Thomson Effect, 440.
Perry (Prof. J.) on the Faure accumulator, 41; on a simplified dispersion-photometer, 45.
Phenol, on the melting-point of derivatives of, 20.
Photometer, on a simplified dispersion, 45.
Physical units, on absolute systems of, 81.
Platinum metals, on some explosive alloys of zinc and the, 152.
Polarization, on the oscillations of the plane of, produced by the discharge of a battery, 79.
Pseudobrookite, on the crystalline form of, 119.
Quartz, on some peculiar crystalline forms of, 122.
Rayleigh (Lord) on the equilibrium of liquid conducting-masses charged with electricity, 184; on an instrument capable of measuring the intensity of aerial vibrations, 186; on the methods for the determination of resistances in absolute measure, 329.
Refraction, on double, produced by electrical influence in glass and bisulphide of carbon, 127.
Refractive index, on the measurement of, 30.
Resistances, on the methods for the determination of, in absolute measure, 329.
Sargant (E. B.) on the dimensions of the magnetic pole in electrostatic measure, 395.
Schmidt (Dr. F.) on the Silurian and Cambrian strata of the Baltic provinces, 150.
Science and metaphysic, 75.
Serpentine, notes on, 477.
Siemens (C. W.) on a new theory of the sun, 480.
Smith (F. J.) on a new form of magnetic torsion-balance and magnetometer, 227.
Solar energy, on the conservation of, 322.
Spectra of metalloids, on the influence of temperature upon the, 406.
Spectral images, on the binocular union of, 315.
Stevens (W. Le Conte) on physiological optics, 312.
Sun, on Dr. Siemens's new theory of the, 400, 478, 480.
Sundell (A. F.) on absolute systems of physical units, 81.
Tasimeter, on the influence of time on the change in the resistance of the carbon disk of Edison's, 115.
Thermometers, on the depression of the zero-point in mercurial, 76; on a simple method for calibrating, 294.
Thermometry, notes on, 57.
Thomson (J. J.) on the dimensions of a magnetic pole in the electrostatic system of units, 225.
Thomson effect, on the, 440.
Tide, on the disturbance of the vertical near the coasts of continents due to the rise and fall of the, 416.
Toluidine, on the melting-point of some derivatives of, 19.
Toluol, on the melting-points of some derivatives of, 11.

Torsion-balance, on a new form of magnetic, 227.
Trowbridge (J.) on the Thomson effect, 440.
Viscosity and density in fluids, on the connexion between, 51.
Vision by the light of the electric spark, on, 313.
Warburg (E.) on the connexion between viscosity and density in fluids, 51.
Wiedemann (Prof. E.) on a property of the coefficient of absorption, 483.
Wiedemann (Prof. G.) on the methods employed for determining the ohm, 258.
Wilson (E.) on the Rhætics of Nottinghamshire, 149.
Wood (S. V.) on the Newer Pliocene period in England, 142.
Wright (C. R. A.) on the determination of chemical affinity in terms of electromotive force, 188.
Wroblewski (S.) on the influence of the quantity of gas dissolved in a liquid upon its surface-tension, 236; on the surface-tension of some liquids in contact with carbonic acid, 327.
Zinc, on some explosive alloys of, and the platinum metals, 152.
Zoisite, on the crystalline form of, 121.

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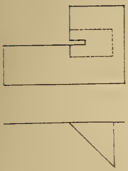

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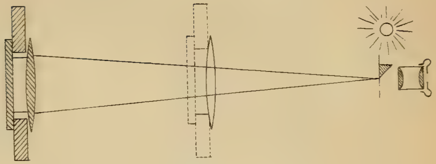

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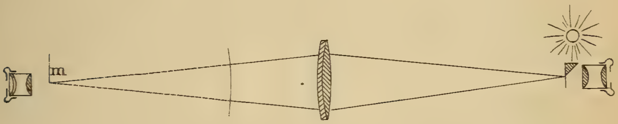

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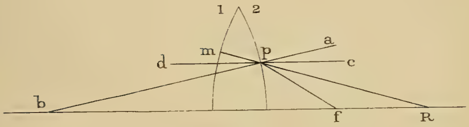

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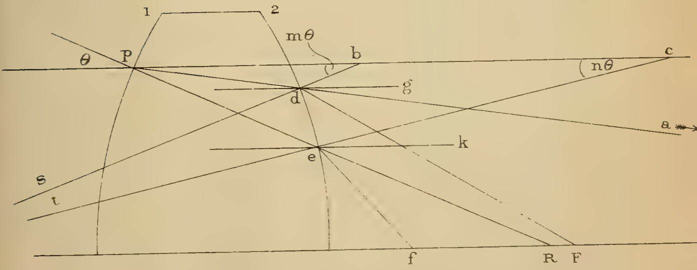

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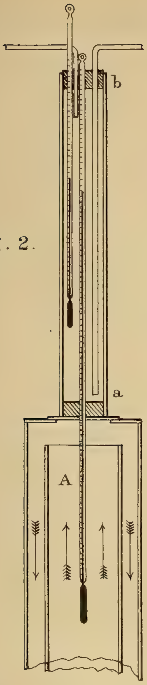

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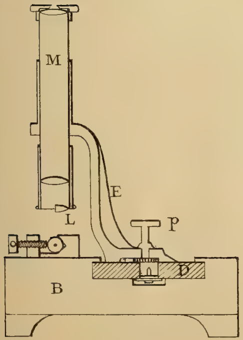

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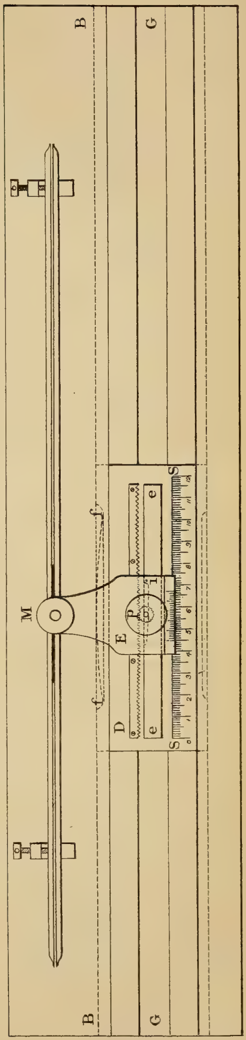

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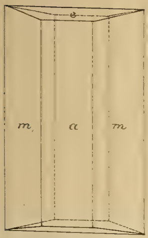

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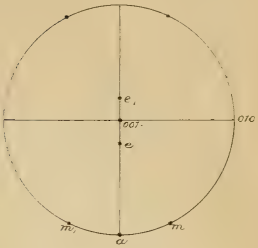

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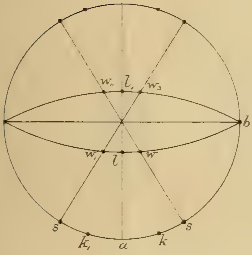

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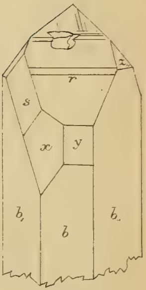

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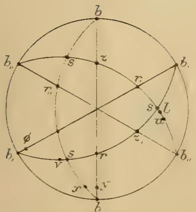

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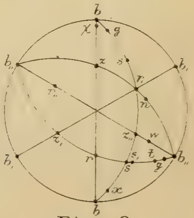

Fig. 6.

Quartz.

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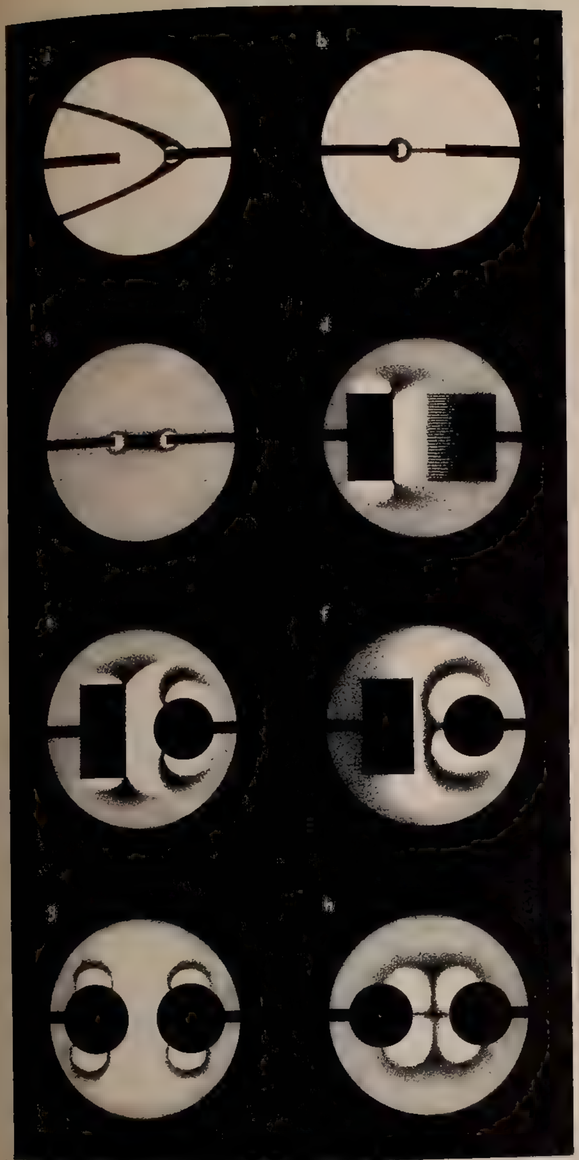

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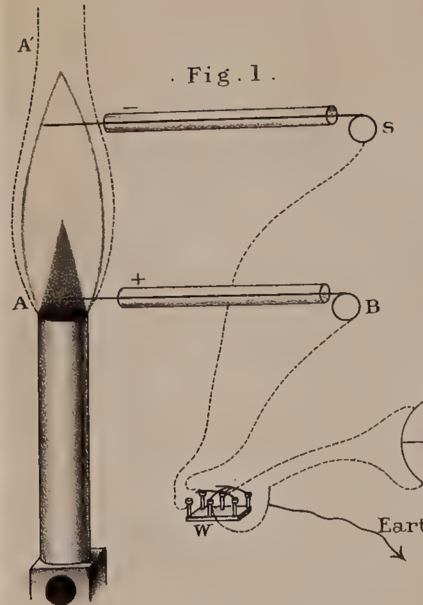

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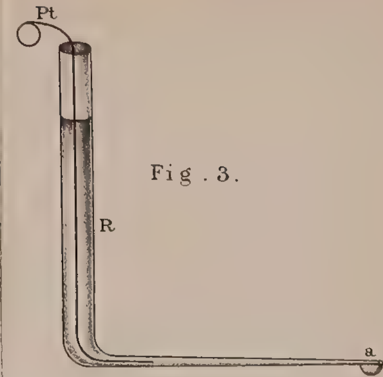

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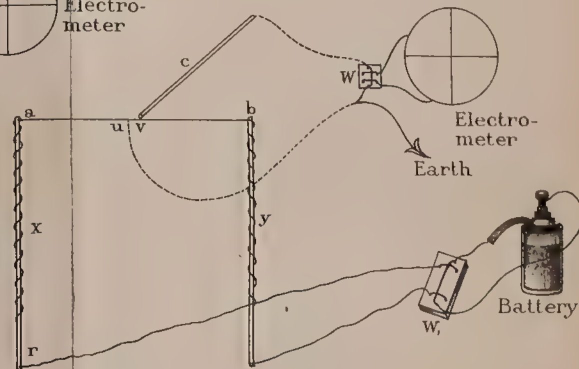

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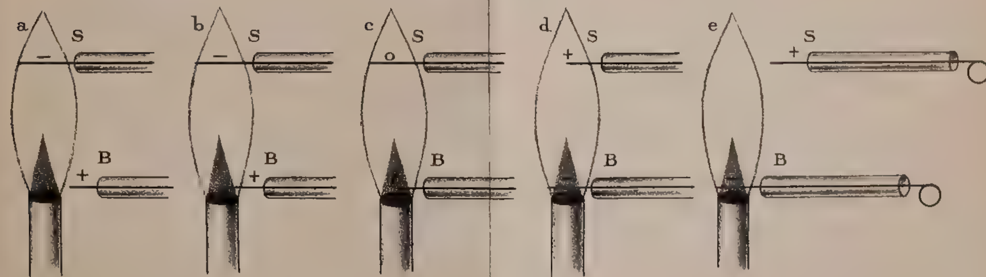

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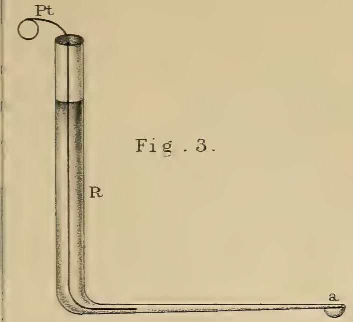

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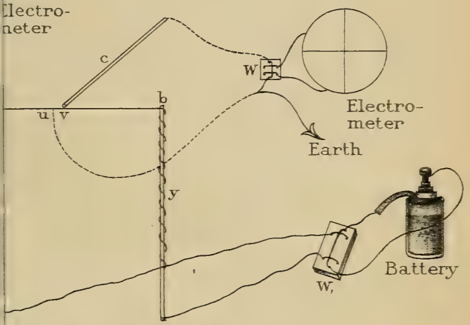

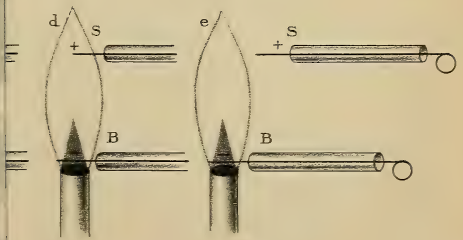

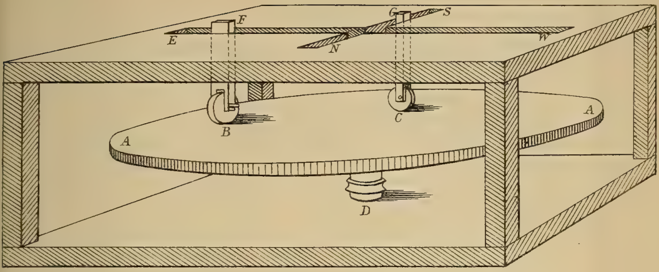

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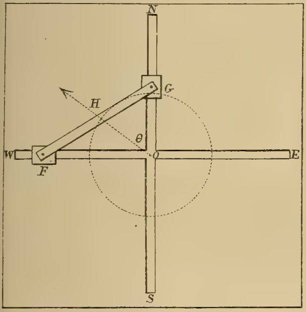

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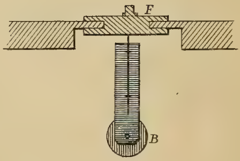

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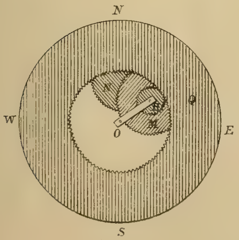

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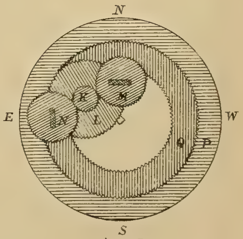

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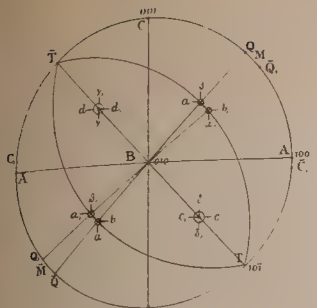

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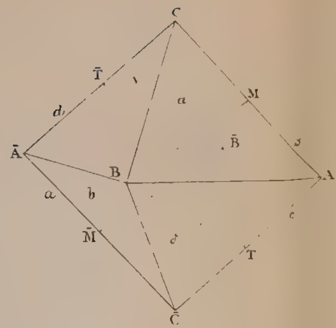

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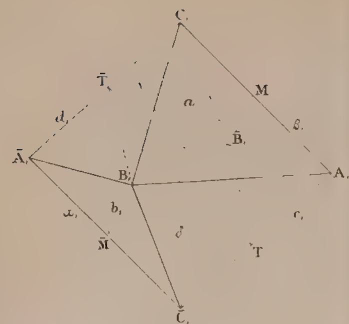

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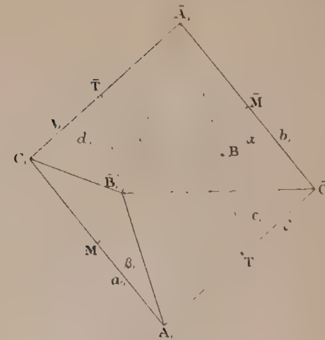

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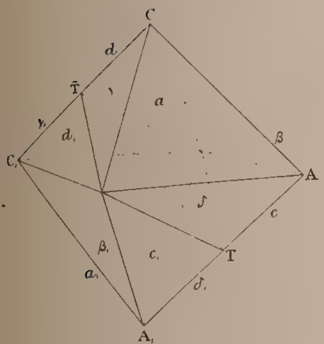

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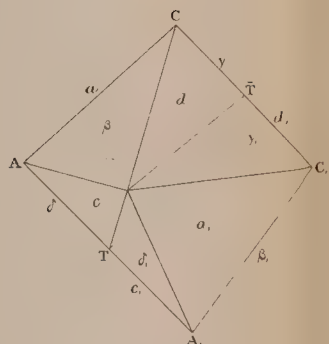

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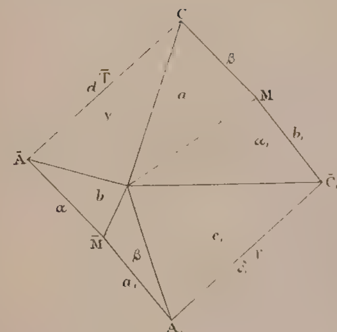

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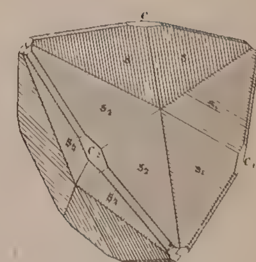

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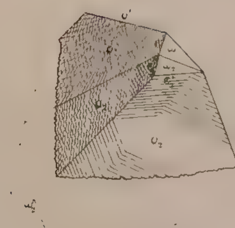

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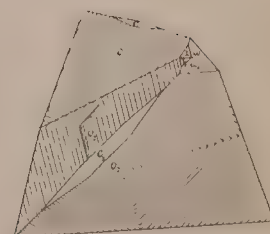
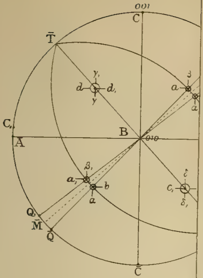


Fig 12

Fletcher del

Copper Pyrites.



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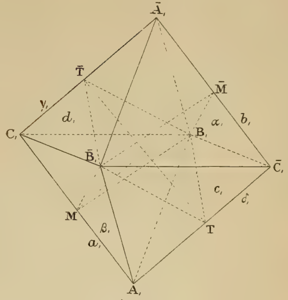

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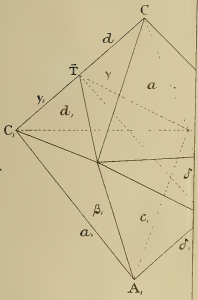

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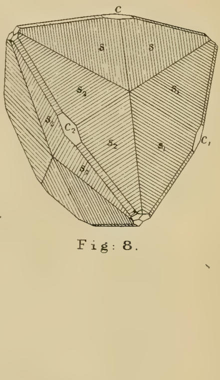

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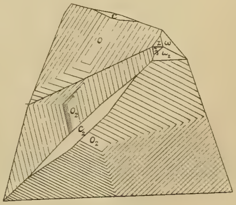

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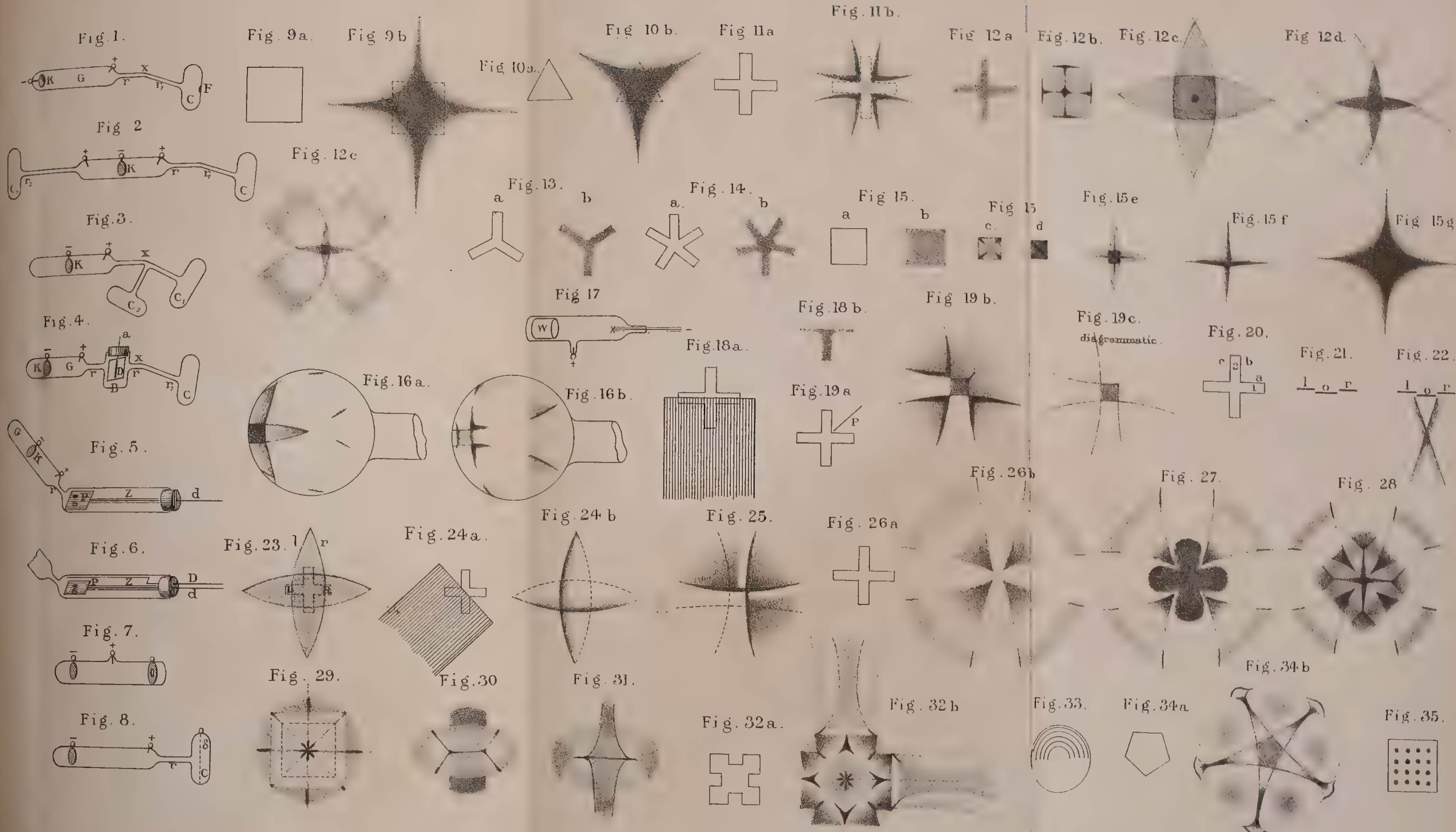

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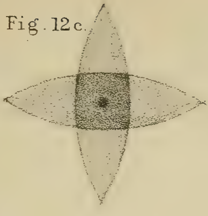
Fig. 12b.

Fig. 12c.



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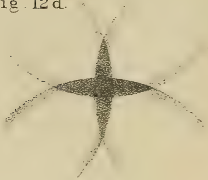

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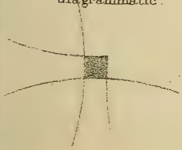
Fig. 15.f.

Fig. 15g.

Fig. 19c.

diagrammatic.



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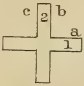

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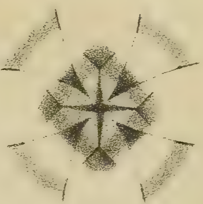

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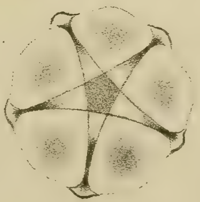

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Fig. 35.

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Errata in No. 87 (September 1882).

Page 217, line 10, for $\frac{dy^2}{dx^2}$ put $\frac{d^2y}{dx^2}$.

— 221, line 4, *for* cos $(2\omega+2\delta)$ *put* sin $(2\omega+2\delta)$.

— 222, line 4, *for* $\cos^2\delta$ *put* $\cos^2 2\delta$.

— 223, line 30, for η read $\frac{g}{a\mu}\eta$ bis.

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